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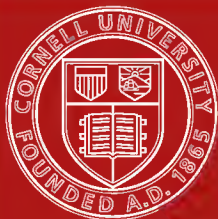
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A B C OF HYDRODYNAMICS

A B C OF HYDRODYNAMICS

BY
LIEUT.-COL. R. DE VILLAMIL
R. ENG. (RET.)⁼

“Next to being right in this world, the best of all things is to be clearly and definitely wrong, because you will come out somewhere. If you go buzzing about between right and wrong, vibrating and fluctuating, you come out nowhere; but if you are absolutely and thoroughly and persistently wrong, you must, some of these days, have the extreme good fortune of knocking your head against a fact, and that sets you all straight again.” HUXLEV.

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TO THE
M e m o r y
OF
LORD KELVIN
WITH
PROFOUND RESPECT
AND IN
ADMIRATION OF HIS CONTRIBUTIONS
TO
HYDRODYNAMICS

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PREFACE

THIS little book has no pretensions to being a treatise on hydrodynamics. It is, as its title implies, only intended as an introduction to the study of that subject. There is not very much that is new in it; some of the quotations are so old that they have been forgotten, and so will appear to the reader of the ordinary text-books as if they were new. What is, I fancy, original is the way in which the matter is arranged and the subject presented. Instead of treating of the movement of a "perfect liquid" and then informing the reader that ordinary liquids behave *quite differently*, I have endeavoured to show that perfect and imperfect (?) liquids follow exactly the same laws, and *under similar conditions* move in a similar manner.

The difficulties are generally supposed to be very great. Sir John Herschel told us that "if there be one part of dynamic science more abstruse and unapproachable than another, it is the doctrine of propagation of motion in fluids, and especially in elastic fluids like the air, even where the amount and application of the original acting forces are known and calculable." These difficulties are, I think, very largely artificial ones. It is well to remember what Dubuat said more than a hundred years ago: "On risqué souvent de se tromper, quand on applique aux fluides les lois du mouvement qui conviennent aux solides." I might even go further and say that, when thinking of liquids, what (to the untrained mind) appears "obvious" or "appeals to common sense" is *very frequently* wrong—more frequently, perhaps, than not. When the student has trained himself to "think in pressures" many difficulties will disappear.

Another difficulty is caused by the assumption of "continuity" in the perfect liquid, a property which certainly does not pertain to mundane liquids. Avanzini saw and pointed out this as long ago as 1804, when he said (*Istituto Nazionale Italiano*, Tomo i., Parte 1): "The physico-mathematicians who, with their investigations on the resistance of fluids, arrived at the ordinary formula, *all supposed, either implicitly or tacitly*—

"1. That the pressure of the fluid *derived from its own weight has no influence on the resistance.*

"2. . . . If the first hypothesis is not true *ordinarily*, it is so, nevertheless, *in the case where there is no vacuum formed*"¹ [*italics added*].

Avanzini's meaning is obviously that gravity has no effect in producing resistance unless there is a vacuum. Put slightly differently, if there is no free surface—*somewhere*—there can be no *inertia* resistance.

Fifty years elapsed, and then Lord Kelvin said, practically, the same thing: "Thus it is that gravity, which could not affect the motion of a liquid entirely filling a rigid closed vessel, will exercise a most important influence on the motion of a liquid contained in an open vessel, and exposing a free surface to the atmospheric pressure."

Some parts of the book are necessarily contentious. I have, however, when I disagree with the ordinary modern teaching, only quoted from authors of position and authority to explain my objections. The quotations are, perhaps, rather full, but I do not think it is fair to take a sentence out of a paragraph and remove it from the context.

In all my arguments and reasoning I have closely followed

¹ "I fisico-matematici, che colle loro, investigazioni intorno alla resistenza de fluidi giunsero a trovare la formula ordinaria, supposero tutti o espressamente o tacitamente.

"1. Che la pressione del fluido nascente dalla sua propria gravità non abbia influsso alcuno sopra la resistenza.

"2. . . . Si la prima ipotesi non è vera generalmente lo è pero nel caso che non succeda vacuo."

I have translated "vacuo" by vacuum; this I define as a *space void of liquid*.

the teaching of Lord Kelvin, and I have not, I trust, stated anything that he would have disapproved of.

The views on viscosity are based on those of Newton. If, as he taught, the resistance due to viscosity is a *resistance to shearing* of the liquid, then, in the last analysis, it must be caused by the rotating liquid molecules rubbing against one another and so generating heat. If my hypothesis of the way the molecules move be considered theoretical and "fantastic," I must plead (1) my belief that "those who refuse to go beyond fact rarely get as far as fact" (Huxley), and (2) that it is not one bit more fantastic than the kinetic theory of gases. Carlyle has said that the tree of knowledge requires periodical shaking; but for the shaking to be any good it must be severe and thorough. I think the hydrodynamical tree will not be hurt by a shaking. By following the train of argument adopted here I trust that a clearer view on the subject will be attained; and to the student I would beg to repeat Professor Osborne's words (*Huxley and Education*): "Do not climb that mountain of learning in the hope that when you reach the summit you will be able to think for yourself: think for yourself while you are climbing."

The purist may object to the style, in consequence of my very liberal employment of emphasis: I trust, however, that the reader will find that what he loses on the swings he gains on the roundabouts.

I wish to acknowledge my great indebtedness to Mr Lewis R. Shorter, B.Sc., not only for reading the proofs, but for his continual help and sympathy, valuable criticism and great assistance in mathematical questions. He is responsible for such "betterment" as gives him almost a tenant right in these pages. My best thanks are also due to Mr Charles Spon for the preparation of the index, which adds value to the little book, as well as for many useful suggestions.

R. DE VILLAMIL.

December 1911.

ABC OF HYDRODYNAMICS

CHAPTER I

RESISTANCE OF LIQUIDS—CONFUSED STATE OF THE SUBJECT

LORD RAYLEIGH in one of his very earliest papers contributed to the *Philosophical Magazine* (1876), said: "There is no part of hydrodynamics more puzzling to the student than that which treats of the resistance of fluids." It is sad to have to admit that, after more than a quarter of a century, this is as true to-day as when the remark was made. My present object is to try and point out the pitfalls to be avoided in this study, as well as to offer a simple and non-mathematical explanation of how the resistance of a liquid is actually caused, many of the difficulties being more apparent than real.

Lord Rayleigh continues: "According to one school of writers, a body exposed to a stream of perfect fluid would experience no resultant force at all, any augmentation of pressure on its face, due to the stream, being compensated by equal and opposite pressure on its rear; and indeed it is a rigorous consequence of the usual hypothesis of perfect fluidity and of the continuity of the motion, that the resultant of the fluid pressures reduces to a *couple* tending to turn the broader face of the body towards the stream."

We see in this remark, "according to one school of writers," that Lord Rayleigh does not commit himself to this view, though he admits that there is something to be said for it, subject to the hypothesis of perfect fluidity and of "continuity of the motion."

The statement that a body moving in a non-viscous or perfect liquid would meet with no resistance, *if unqualified*, is in direct contradiction to the teaching of Newton. If, however, it be qualified by the addition that the liquid must be of *infinite dimensions in every direction*, then it is, I believe, mathematically true. This is only saying, in a different way, that all motion must be strictly continuous.

The whole of this paper is so very interesting that it will be quoted from freely: "It was Helmholtz who first pointed out that there is nothing in the nature of a perfect fluid to forbid a finite slipping between contiguous layers, and that the possibility of such occurrence is not taken into account in the common mathematical theory, which makes the fluid flow according to the same laws as determine the motion of electricity in uniform conductors."

There is, of course, nothing in the *nature* of a perfect fluid to "forbid slipping," but in the mathematical theory all motion is assumed to be *strictly continuous*: since the slipping would imply *discontinuity*, it is barred by the premises. As the mathematicians' fluid is always assumed as of *infinite dimensions*, it is obvious that no crack or "rift" could be made in it, *as it is already occupying the maximum of space possible*.

Again: "Moreover, the electric law of flow (as it may be called for brevity) would make the velocity infinite at every sharp edge encountered by the fluid; and this would require a negative pressure of infinite magnitude.

"It is no answer to this objection that a mathematical sharp edge is an impossibility, inasmuch as the electric law of flow would require negative pressure in cases where the edge is not perfectly sharp."

This difficulty is more apparent than real. It is quite true that a mathematical sharp edge is an impossibility, but so is an infinite velocity, or a negative pressure of infinite magnitude. It will be quite sufficient to say that the sharper the edge the greater will be velocity of flow of the liquid past it, and, consequently, the greater will be the magnitude of the negative pressure there; but to this should also be

added, "and the greater the *plus* pressure *somewhere else*," for a great velocity necessarily postulates a great pressure *somewhere*.

The view I have taken up is that held by Lord Kelvin, who wrote in 1894 (*Nature*):

"§ 1. The doctrine that 'discontinuity,' that is to say, finite difference of velocity on two sides of a surface in a fluid, would be produced if an inviscid incompressible fluid were caused to flow past a sharp edge of a rigid solid *with no vacant space between the fluid and the solid*, was, I believe, first given by Stokes in 1847.

"It is inconsistent with the now well-known dynamical theorem that an incompressible fluid initially at rest, and set in motion by pressure applied to its boundary, acquires *the* unique distribution of motion throughout its mass, of which the kinetic energy is less than that of any other motion of the fluid with the same motion of its boundary.

"§ 2. The reason assigned for the formation of a surface of finite slip between fluid and fluid was the infinitely great velocity of the fluid *at* the edge, and the corresponding negative—infinite pressure—implied by the unique solution, *unless the fluid is allowed to separate itself from contact with the solid*. This an inviscid incompressible fluid certainly would do, unless the pressure of the fluid was infinitely great everywhere except at the edge."

It will be well here to point out that one must be very careful in speaking of *negative* pressure, since, in the last analysis, it will be found that the negative sign is only apparent: speaking generally, there can be no such thing as *negative* pressure. To explain what I mean, suppose a small lamina to be moving broadside-on in a liquid—say water—at such a depth of immersion that the hydrostatic pressure would be 5 lbs. per square inch. At a moderately high velocity the pressure on the leading face of the lamina, *near the edge*, might very easily be *minus* 5 lbs. per square inch. This is not, however, in reality a negative pressure, since the pressure at the surface of the water is not *zero*, but roughly 15 lbs. per square inch: 5 lbs. *minus* pressure

is thus in reality $15-5=10$ lbs. *plus* pressure. This will be considered and become more evident later, when the subject is more developed. That there can be no *negative* pressure must not be taken too literally; ordinary mundane liquids are certainly capable of resisting tension, and under some exceptional circumstances actually do so. Whether a perfect liquid, which offers no resistance to shearing, *could* be under tension appears questionable.

Lord Rayleigh having said: "... It is well known that in practice an obstacle does experience a force tending to carry it down stream, and of magnitude too great to be the direct effect of friction; while in many of the treatises calculations of resistance are given leading to results depending on the inertia of the fluid without any reference to friction," at last comes to the conclusion that "the application of these ideas to the problem of the resistance of a stream to a plane lamina immersed transversely amounts to a justification of the old theory, as at least approximately correct."

This leaves the subject in an extremely nebulous state; but the paper is full of exceedingly interesting and useful details, some of which will be referred to later.

Another distinguished scientific author, Dr Fleming, in his charming little book on *Waves and Ripples*, after speaking of liquid resistance and a "perfect" fluid, says: "It is obvious, from what has been said, that if a perfect fluid did exist, it would be impossible by mechanical means to make eddies in it; but if they were created, they would continue for ever, and have something of the permanence of material substances."

It is certainly not "obvious" to the ordinary reader: it is, on the contrary, absolutely questionable; for in 1887 Lord Kelvin published his classical paper "On the Formation of Coreless Vortices by the Motion of a Solid through an Inviscid incompressible Fluid," in which he showed how the vortices would be formed. In 1904 also he stated: "After many years of failure to prove that the motion in the ordinary Helmholtz circular ring is stable, I came to the conclusion that it is essentially unstable, and that its fate must be to become dissipated as now described."

When experts like these disagree the subject is not made clearer for the student.

A few years ago Dr Hele-Shaw, in his lecture on "The Motion of a Perfect Liquid," said: "Probably one of the most perplexing things in engineering science is the absence of all apparent connection between the higher treatises on hydrodynamics and the vast array of works on practical hydraulics. The natural connection between the treatises of mathematicians and experimental researches of engineers would appear to be obvious, but very little, if any such connection, exists in reality, and while at every step electrical applications owe much to the theories which are common to electricity and hydromechanics, we look in vain for such applications in connection with the actual flow of water."

That this chasm exists is unfortunately only too true; but is it all the fault of the engineers? It does not appear to be so.

Dr Hele-Shaw most rightly says: "All scientific advance in discovering the laws of Nature has been made by first simplifying the problem and reducing it to certain ideal conditions"; but to this he adds, "and this is what mathematicians have done in studying the motion of a liquid."

Now, have the mathematicians "simplified" the problem? Unfortunately, from the hydraulic engineer's point of view, that is what they have *not* done: they have actually complicated matters; so much so that many engineers look upon this "perfect" liquid as a perfectly useless toy invented by, and for the delectation of, the mathematicians.

It is evident that this is not quite a fair way of looking at a "perfect" liquid, the only assumed properties in which it differs from an ordinary liquid being that it is inviscid, or frictionless, and incompressible. It is certainly impossible, but its assumption is no more absurd than that of a frictionless pulley, or a frictionless crane.

When engineers commence the study of mechanics they are introduced to the *frictionless* pulley and are taught that its "advantage" is, say, 4 to 1, or that a certain *frictionless* crane has an "advantage" of 40 to 1. It is only later that

they learn that friction modifies these results more or less. Why is not the same method adopted when treating of liquids? Dr Hele-Shaw says: "The reason for this appears to be the immense difference between the flow of an actual liquid and that of a perfect one *owing to the property of viscosity*." It is difficult to accept this as the correct explanation. If, however, Dr Hele-Shaw's reason were the correct one, it would surely be the very strongest justification for the neglect which he complains of. It would clearly be a waste of time for an engineer to study the movements of a perfect liquid, only to be told at the end that an ordinary liquid behaved in a *totally different manner*. No! a most emphatic negative reply must be given to this; for the only difference due to viscosity between a perfect liquid and water (which is practically incompressible) is that the particles of the former can have no *tangential* stress applied to them, whilst those of the latter can. You cannot, therefore, *rotate* the particles of a perfect liquid, so that all motion of a perfect liquid must of necessity be "irrotational."

It is true that the mathematicians, by their assumption of an inviscid (or, as they call it, "perfect") liquid, *have*, so far, "simplified" the problem, but they have at the same time introduced a special complication by the further assumption that this liquid is of *infinite dimensions*; and it is in this assumption that the cause lies of the discrepancy in the behaviour of the perfect fluid as compared with the real fluid.

The motion of an infinite liquid is (at times) quite different from that of water, which is *not* infinite in extent, and which ordinarily has a free surface: so much so that the theory "appears to be very tolerably complete and affords the means of calculating the results to be expected in almost every case of fluid motion; but while in many cases the theoretical results agree with those actually obtained, in other cases they are altogether different" (Osborne Reynolds). With a sound theory, *under similar conditions*, the flow of a viscid liquid should not be *very* different from that of a perfect one: not much more than, say, the motion of an ordinary pulley differs from a theoretically frictionless one.

What strikes one as extraordinary is that Dr Hele-Shaw should have been the man who first, by suitable means, caused a viscid fluid *to flow like a perfect one*: also, *mirabile dictu*, far from friction being a hindrance, he found water was *not viscid enough* for his purpose, and so he had to employ glycerine. If a viscid liquid be compared with the perfect liquid, it *must be under exactly similar conditions, especially as regards the presence or absence of a free surface*.

It will be well here to define the terms “rotary” and “rotational,” since they may be, and frequently are, employed in two different senses. They will be invariably used here as implying rotation of the *particles* about an axis. When the particles move in a circle, or other closed curve, the motion will be called “cyclic”: cyclic motion may, of course, be “rotational” or “irrotational,” according to whether the particles are turning on their axes or not. Many writers unfortunately use the terms very loosely; sometimes as described here and sometimes as “rotary *or* vortex.” This leads to endless confusion; since an “irrotational vortex” would then be a contradiction of terms.

To show that I have not exaggerated the confused state of the subject, I will close this chapter with a quotation from Mr Lanchester’s *Aerodynamics*, which is a very modern treatise and, probably, contains most of the latest views: I have italicised the parts to which I particularly wish to draw attention:

“§ 98. The deficiencies of hydrodynamic theory have already been pointed out in several instances and partially discussed. The forms of flow that result *from the assumption of continuity* and the equations of motion bear in general *but scant resemblance to those that obtain in practice*, and it is *not altogether easy to account for the cause of failure*.

“If an actual fluid behaved *anything like the ideal fluid of theory*, the *necessity for the ichthyoid form* would not exist (?); any shape, however abrupt, short of producing cavitation, would give rise to stream-line motion [?] and be *destitute of resistance*. The *actual phenomena* of fluid resistance, discussed in the two previous chapters, is characterised by features which

at present are *not capable of complete elucidation* by analytic methods."

In the next chapter we will consider what is implied by the assumption that a fluid is of *infinite extent*, as this is the key which will unlock many difficulties and enable us to reconcile many statements which appear, at first sight, absolutely contradictory.

SUMMARY

The subject of liquid resistance appears to be in a very confused state. This seems to be due to the assumption of the mathematicians' "perfect" liquid, which is supposed to be not only frictionless but also of infinite dimensions, and which consequently *has no free surface*, and the comparison of this liquid, without a free surface, with an ordinary liquid, like water, which *has a free surface*—conditions which are *not similar*.

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CHAPTER II

A LIQUID OF INFINITE DIMENSIONS

IF a liquid (supposed incompressible)¹ be of *infinite* extent in every direction, it is evident that it is 'occupying the *maximum* space possible, and that no particle could move without some other particle moving away to make room for it. Any separation of the particles would be, obviously, impossible. All motion in it would be, in fact, *perfectly continuous*, and all "discontinuity" would be not only non-existent but impossible. All motion of the liquid would be what is called "by obligation." If a body moving in this liquid were *suddenly* stopped, all motion in the liquid would cease *instantly*. Under these circumstances (as the mathematicians have shown), if the liquid were inviscid, *all resistance would be impossible*, since the sum of all the anterior pressures would exactly balance that of all the posterior pressures: in other words, "it would be found that an object of any shape wholly submerged in the fluid could be moved about in any way without experiencing the least resistance" (Dr Fleming, *Waves and Ripples*). This will not be disputed by anyone.

It is not even absolutely necessary to assume that the liquid is quite infinite in extent for this to be true. Since a stress requires time for its transmission to a distance, and since it also takes a definite time for the formation of a

¹ When I speak of a liquid as being an *incompressible* fluid, it must be understood that I mean that it is *nearly* incompressible, compressibility being a negligible factor. If one assumed (as is commonly done) a liquid to be *absolutely* incompressible, it is difficult to imagine how energy could be communicated from one particle to another of it.

vortex, it follows that if the free surface be so far away that the stress cannot travel to this free surface in the time required for the formation of the vortex, then the actual result would be the same as if the liquid were *infinite*. For example, if we imagine that the velocity with which a stress can travel in a liquid to be 186,000 miles in a second—it is hardly probable that it could possibly exceed this—and if we suppose that it takes even half a second to form a vortex—which is probably not less than ten times the actual period—then, if the body moving in the liquid were 93,000 miles from the free surface, there would be no *inertia* resistance.

If the non-viscous liquid, however, were *not* of infinite extent—if it had a free surface within a reasonable distance—if, in fact, it were like the Atlantic Ocean—then resistance would be quite possible, and the behaviour of the liquid would not be unlike that of water, for the flow would not be, *necessarily*, one “by obligation.”

This is the view that was held by Lord Kelvin, who stated (*Nature*, 1894): “A stiff circular disc of 10-inch diameter and $\frac{1}{16}$ of an inch thick in its middle, shaped truly to the figure of an oblate ellipsoid of revolution, would cause a vacuum¹ to be formed all round its edge, if moved at even so small a velocity as *1 foot per second* [*italics added*] under water of any depth less than 63 feet, if water were inviscid; and at greater depths the motion would, on the same supposition, be wholly continuous, with no vacuum, and would be exactly in accordance with the unique minimum of energy solution”; and later:

“In the case of our circular disc, it would be circular vortex rings that, if the water were inviscid, would be shed off from its edge when the depth is less than 63 feet. If the depth is very little less than 63 feet, these rings would be exceedingly fine, and would follow one another at exceedingly short intervals of time. Thus quite close to the edge there would be something somewhat like to Stokes’ ‘rift,’ but with a rapid succession of vacuum rollers, as it were, and *no slipping* between the portions of the fluid on its two sides.”

¹ Single word to denote space vacated by water.

Since it is difficult to picture to oneself a fluid of infinite extent—even if it be possible at all—it will perhaps be better to put the case in a way which is easier to grasp. Suppose an inviscid, incompressible liquid to completely fill a closed tank whose sides are *perfectly rigid*. If the tank be assumed to be large, the case will, in essence, be similar to that of a liquid of *infinite* extent, since the liquid will have no free surface, and it will be occupying the *maximum* space possible. Any body moving in this liquid would meet with no resistance of any kind whatever: all anterior pressures would be exactly balanced by the posterior pressures. There would be no, what we may call, “free” flow, since all flow would be “by obligation”; and if the body were stopped, all motion of the liquid would cease *instantly*.

If the body moving in this liquid were a thin lamina, as its velocity increased, *so would the pressure on the walls of the tank*. Even at very moderate velocities this pressure might become enormous; whilst if the lamina had a mathematical sharp edge, the pressure on the tank walls would necessarily be *infinite*. This question has been very fully and ably treated by Professor Lamb in his *Hydrodynamics*.

Next, let us imagine the roof of the tank to be removed. The liquid now has a free surface, and any motion in it would not, necessarily, be one “by obligation,” since the pressure on the tank walls could not be sensibly increased, but would resemble that of an ordinary liquid, like water. Coreless vortices would be formed, as has been pointed out by Lord Kelvin in his papers referred to.

It is not even necessary that the whole of the roof of the tank should be removed. If a small hole were bored in it and a stand-pipe were attached, on moving the lamina, coreless vortices would still be formed, whilst the level of the liquid in the stand-pipe would rise.

Some writers appear to assume that what is true in the closed tank will be equally true in the open tank. Mr Lanchester (*Aerodynamics*), referring to this, says:

“When a body propelled through an incompressible fluid, contained within a fixed enclosure, experiences resistance to its

motion, the force exerted by the body on the fluid does not impart momentum to the fluid, but is transmitted instantly to the confines of the fluid, however remote, and is wholly borne by its boundary walls." This is a very clear statement of the case, and Mr Lanchester refers to it in his work as the "principle of no momentum." Four pages later he continues: "When a body of fish-shaped or *ichthyoid* form travels in the direction of its axis through a frictionless fluid [no mention made of "contained within a fixed enclosure"] there is no disturbance in its wake. Now we have seen that *in any case* [italics added] the fluid as a whole *receives no momentum*, so that it is perhaps scarcely legitimate to argue that there is no resistance *because* there is no communication of momentum, although this is a common statement. It is clear, however, that if there is no residuary disturbance, there is no necessary expenditure of energy, and this equally implies that the resistance is nil."

The usual assumption is here frankly stated. The argument is something as follows:—

1. A fish in a *fixed enclosure*, filled with perfect liquid, when moving would generate no momentum.

2. *Therefore, in any case*, "the fluid as a whole receives no momentum." The cases are not similar and the one does not follow from the other. Of course momentum can be used in two senses. If Mr Lanchester had said that the fish could generate no *linear momentum*, then no fault could, perhaps, have been found with his statement. It is contended here that *no* body can generate it in a liquid—perfect or imperfect—for a liquid does not obey Newton's second law of motion. This will be referred to later. There is, however, another kind of momentum—cyclic momentum—which a body *can* generate in a liquid. It is the generation of this momentum which *causes resistance in liquids*, and which would cause resistance in a "perfect" liquid, if such a thing existed.

As if the subject were not sufficiently complicated already, Mr Lanchester also says: "There are certain cases in which the principle of the *continuous communication of momentum* [linear is implied] *can be applied*. A most striking example is

to be found in the theory of marine propulsion founded by Rankine and Froude." . . . "In general it would appear that the Newtonian¹ method is applicable in cases *where the volume of the fluid handled is great*, but where the impressed velocity is small in comparison with the velocity of motion, and *where there are well-defined conditions on which to compute the amount of fluid dealt with.* . . ."

If this were not stated in all seriousness one would feel inclined to treat it as a joke. Surely the volume of water handled by the *Mauretania* ought to be sufficient to comply with the pushing requirements. There are also *well-defined conditions for computing the amount of fluid dealt with.* In this case, however (according to Mr Lanchester), the small propeller can apparently do what the leviathan *can not*.

¹ It is not exactly evident why Mr Lanchester refers to this as the "Newtonian method." I cannot see that it is in the least in accord with the teaching of Newton, any more than it is with the writings of Lord Kelvin. In the *Principia* (Book II. Sec. vii. Prop. xxxvi. Problem viii. Cor. 7—Motte's translation) Newton refers to the flow of water past a small circular disc, and he shows that the pressure may be expressed, graphically, by a conoid of revolution, as in the sketch. The pressure at D is represented by CD, the hydrostatic pressure; and the curve of pressure by AC and CB. Since this conoid is convex, he says the *total pressure*, on the disc, *will exceed one-third of the hydrostatic pressure.*

In Cor. 8 he points out that, as the angles at A and B are *less than right angles*, the *total pressure* will be *less than that represented, graphically, by a sphaeroid*: in other words, that the *total pressure* will be *less than two-thirds of the hydrostatic pressure.*

In Cor. 9, Newton further says that when the disc is very small the total pressure will be "very nearly equal" to *one-half of the hydrostatic pressure.*

These statements are made subject to the condition that the velocity of flow of the water past the disc may be represented by $v = \sqrt{2gh}$, where h = depth of immersion of the disc.

How all this accords with what Mr Lanchester calls the "Newtonian method" is not at all clear to me. If a plate *drives* water it would appear to be necessary that the pressure on the "driving surface" should be *in excess of the hydrostatic pressure—and not the reverse.* Besides, the "volume of fluid handled" here is *not* "great," but, on the contrary, *very small.*

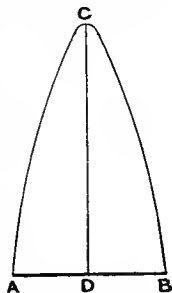


FIG. 1.

There can be only *one law* for *all bodies*. Either a body *can* push water or it *can not*; and by "push" is meant to generate linear momentum by pressure. I know of no evidence that a propeller does drive the water backwards. Water *goes* back, of course, and cyclic momentum is generated; but all the longitudinal acceleration of the water is produced *in front of the propeller*—i.e. towards the ship—and not behind it.

Mr Lanchester's fish, if swimming near the surface of the perfect liquid, would undoubtedly cause waves.

I cannot do better than close this chapter by an extract from Lamb's *Hydrodynamics* (1895):

"73. We have, in the preceding pages, had several instances of the flow of a liquid round a sharp projecting edge, and it appeared in each case that the velocity there was infinite. This is indeed a necessary consequence of the assumed irrotational character of the motion, whether the fluid be incompressible or not, as may be seen by considering the configuration of the equipotential surfaces (which meet the boundary at right angles) in the immediate neighbourhood.

"The occurrence of infinite values of the velocity may be avoided by supposing the edge to be slightly rounded, but even then the velocity near the edge will much exceed that which obtains at a distance great in comparison with the radius of curvature.

"In order that the motion of a fluid may conform to such conditions, it is necessary that the pressure at a distance should greatly exceed that at the edge. This excess of pressure is demanded by the *inertia* of the fluid, which cannot be guided round a sharp curve, in opposition to centrifugal force, except by a distribution of pressure increasing with a very rapid gradient outwards.

"Hence, unless the pressure at a distance be very great, the maintenance of the motion in question would require a negative pressure at the corner, such as fluids under ordinary conditions are unable to sustain.

"To put the matter in as definite a form as possible, let us imagine the following case. Let us suppose that a straight

tube, whose length is large compared with the diameter, is fixed in the middle of a large closed vessel filled with frictionless liquid, and that this tube contains, at a distance from the ends, a sliding plug, or piston P, which can be moved in any required manner by extraneous forces applied to it. The thickness of the walls of the tube is supposed to be small in comparison with the diameter, and the edges, at the two ends, to be rounded off so that there are no sharp angles. Let us further suppose that at some point of the walls of the

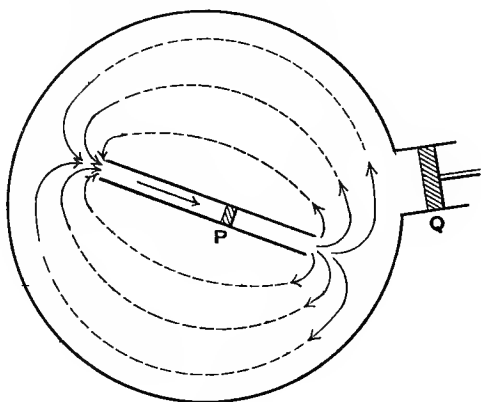


FIG. 2.

vessel there is a lateral tube, with a piston Q, by means of which the pressure in the interior can be adjusted at will.

“Everything being at rest to begin with, let a slowly increasing velocity be communicated to the plug P, so that (for simplicity) the motion at any instant may be regarded as approximately steady. At first, provided a sufficient force be applied to Q, a continuous motion of the kind indicated in the diagram on p. 83 will be produced in the fluid, there being, in fact, only one type of motion consistent with the conditions of the question. As the acceleration of the piston P proceeds, the pressure on Q may become enormous, even with very moderate velocities of P; and if Q be allowed to yield, an annular cavity will be formed at each end of the tube.”

Besides this, "Gravity, which could not affect the motion of a liquid entirely filling a rigid closed vessel, will exercise a most important influence on the motion of a liquid contained in an open vessel, and exposing a free surface to the atmospheric pressure" (Lord Kelvin, "Notes on Hydrodynamics," *Cambridge and Dublin Mathematical Journal*, 1849).

This question of the perfect liquid being assumed as of *infinite extent* has been referred to at considerable length; but it is a very important one, and one which is, frequently, not properly grasped. Even that scientific giant, Lord Kelvin, was nodding when he wrote his paper on coreless vortices, for he there assumed the fluid to be "of infinite extent in all directions." He admitted in a letter, before his death, that his meaning was that "the infinite liquid extends through all space *except certain given portions* which are the hollows of the supposed coreless vortices": in other words, that the liquid was *nearly* infinite only. I am afraid this does not get over the difficulty, though it certainly shifts it away another step. To form the coreless vortices it would be necessary to shift a (virtually) infinite column of liquid a finite distance in a finite time, which is unimaginable.

SUMMARY

"All the forces which are observed in Nature to act upon the mass of a liquid at rest, whatever may be the agencies to which it is subjected, are such that if the liquid be enclosed in a fixed envelope they cannot disturb its equilibrium, but are in all cases balanced by the resistance which the fluid pressure experiences from the bounding solid, . . . and therefore, in all cases in which the form of the bounding surface is susceptible of alteration by the pressure of the fluid, the forces through the mass will, by the effect they may thus produce on the form of the bounding surface, exercise an indirect influence on the motion which takes place within it. Thus it is that gravity, which could not affect the motion of a liquid entirely filling a rigid closed

vessel, will exercise a most important influence on the motion of a liquid contained in an open vessel, and exposing a free surface to the atmospheric pressure" (Lord Kelvin, *Cambridge and Dublin Mathematical Journal*, 1849).

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- H. LAMB, *Hydrodynamics*.
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NEWTON, *Principia*, vol. ii.

CHAPTER III

MOVEMENT OF A LIQUID AND THE LAW OF FLOW

HAVING now a clear idea of the difference between a "free" flow and a flow "by obligation," it will be well to consider what a liquid is, and what are the laws regulating its movement.

Newton defined a fluid as "any body whose parts yield to any force impressed on it, and, by yielding, are easily moved among themselves." He seems to have laid stress on the difference between a solid and a liquid: that the former "resists" a force, whilst a liquid "yields" to it, a liquid being an incompressible fluid.

The *Encyclopædia Britannica* defines—"a fluid, as the name implies, is a substance which flows, or is capable of flowing." This may appear a little tautological; but it conveys a clear idea, though it might be necessary to precise what "flowing" means.

A careful examination of the subject will satisfy anyone that a fluid cannot be "pushed" or "driven": a body moving in a liquid can only *divide it*. This was pointed out by Dr Wallis, who was a contemporary of Newton and who is referred to in the *Principia*, with Sir Christopher Wren and Huygens, as "the greatest geometers of our times." The Chevalier Dubuat, in referring to this point, says: "Quand un corps résiste à l'action d'un courant, le fluide se dévie à une certaine distance en avant de lui, et qu'il se forme *une espèce de proue fluide*, dans laquelle les filets, sans perdre toute leur vitesse, en ont cependant moins que le reste du fluide, dans le sens du mouvement général." In short, a fluid, as its name

implies, *flows*: it *can only* flow, and it *must* flow, from any region of higher pressure to any region of lower pressure. You can *induce* a fluid, by suitable means, to flow in any given direction, but you cannot mechanically *drive it* in any direction.

It is well that I should explain exactly what I mean. It may be objected that water is forced, or "driven out," of a squirt, and that, like a solid, it obeys Newton's second law of motion. This is not so, pressure being equal in all directions. "Every particle of the stagnant water is equally pressed on all sides, and, yielding to the pressure, tends all ways with an equal force, whether it descends through the hole in the bottom of the vessel, or gushes out in a horizontal direction through a hole in the side" (*Principia*). If a small hole be bored in the side of the squirt, the water will flow out just as readily at right angles as along the axis of the piston; if the packing be loose, it will even flow as readily *backwards* as forwards. It cannot be contended that there is any force which *drives* the water at right angles or backwards.

It has been objected to me (by a Professor of Mathematics) that the force driving the water in the squirt is not necessarily *in the direction of the motion of the piston*; that the real force is the *resultant* of many forces, and that no attempt has been made to calculate the direction of this resultant, etc. etc. This is quite true; but if no attempt has been made to calculate the *direction* of this resultant, it is for the very best of reasons, viz., that there is *not*—and cannot be—*any* resultant. Whether the stress be applied from the N., S., E., W. Zenith or Nadir, the result is exactly the same; each particle of water is equally stressed in every direction. If the force applied to the piston be equal to 10 lbs. per square inch, every particle of water will have a pressure equal to 10 lbs. per square inch over the *whole of its surface*. If we could imagine that the force was applied from the north, and that the resultant force was calculated to pass through some particular hole in the syringe; if we next applied the force from the south—or any other point of the compass—the *resultant* could hardly be the same; yet the water would flow out in *exactly the same manner*. There being no resultant, there is

no force "driving" the water out. All that you do in the squirt—all that you *can* do—is to produce a region of higher pressure in the instrument, and the liquid then flows from this region to *any region of lower pressure*. Similarly, if you pull up the plug of a bath, or turn on a water-tap, you produce a region of *lower* pressure, and the water flows towards this.

I may appear to have rather laboured this point, but the statement that a liquid *does not obey Newton's second law of motion* is generally received with a smile of surprise, if not of contempt. The assertion having been made, it was as well to be precise. It is not denied that *each individual particle* obeys this law: what is maintained is that in a body of water the particles in their "corporate capacity" do not obey this law. The two things are quite different; and parallel cases, in everyday life, amongst men might be quoted.

In the first place, Newton never said that a *liquid* obeyed this law, and his teaching in the *Principia* appears to be distinctly against it. To take a few examples:—Prop. xli. theorem xxxiii.: "A pressure is not propagated through a fluid in rectilinear directions, unless where the particles of the fluid be in a right line." Prop. xlii. theorem xxxiii.: "All motion propagated through a fluid diverges from a rectilinear progress into the unmoved spaces."

Other propositions might be quoted, but I should like specially to point out that when he is speaking about a circular disc moving in water, at the *speed corresponding to the depth* ($v^2 = 2gh$), he says the pressure on its leading surface would be only about half the hydrostatic pressure (Book II. Sec. vii. Prop. xxxvi. Cor. 9).

The statement about "flow," made previously, admits of no exceptions; for when there are, apparently, cases where it does not apply, it will be found that there is something which is *preventing* the flow. For example, a gas may be under pressure in a bag and so not able to flow to the region of lower pressure outside; if the pressure be increased, the bag may burst, and then the flow will take place. This example may appear childish, but there are many cases where the surface of a liquid acts like the material of a gas bag and so

prevents the flow of the liquid, as long as the pressure is not too high ; if the pressure increases too much the film breaks and the liquid flows.

If we admit that a fluid flows, that it *can* only flow, and that it *must* flow from a region of higher to one of lower pressure, what is the law regulating this flow ?

The law connecting the pressure in a liquid and its velocity, when the flow is steady, can be expressed by the very simple formula

$$p + \frac{1}{2}\rho v^2 = \text{constant},$$

where p = pressure in the liquid, ρ = density, and v = velocity ; or, expressed slightly differently,

$$P_0 = p + \frac{1}{2}\rho v^2,$$

where P_0 = pressure at zero velocity. "By steady motion I mean motion which at any and every time is precisely similar to what it is at one time" (Lord Kelvin, *Vortex Statics*).

Quoting from Lord Rayleigh's paper previously referred to : "The relation between the velocity and pressure in a steady stream of incompressible fluid may be obtained immediately by considering the transference of energy along an imaginary tube bounded by stream-lines.

"In consequence of the steadiness of the motion, there must be the same amount of energy transferred in a given time across any one section of the tube as across any other.

"Now, if p and v be the pressure and velocity respectively at any point and ρ be the density of the fluid, the energy corresponding to the passage of the unit of volume is $p + \frac{1}{2}\rho v^2$, of which the first term represents the *potential* and the second the *kinetic* energy ; and this $p + \frac{1}{2}\rho v^2$ must retain the same value at all points of the same stream-line."

This may be considered *the* formula in hydrodynamics which especially interests engineers, and it will be frequently referred to. Not only is the energy (potential + kinetic) constant all along the *same* stream-line, but it is the same for *all* the stream-lines *in the same horizontal plane*.

"It is further true, though not required for our present purpose or to be proved so simply, that $p + \frac{1}{2}\rho v^2$ retains a con-

stant value, not only *in the same stream-line*, but also when we pass *from one stream-line to another* [? in the same horizontal plane], provided that the fluid flows throughout the region considered in accordance with the electric law" (Lord Rayleigh).

A natural corollary of this is that, in the same horizontal plane *all the parts of a liquid in steady motion* must be under *lower pressure* than those parts which are at rest, or are moving at a lower velocity. Also, *the greater the velocity the lower will be pressure*.

To put the matter familiarly, if we consider the potential energy of the liquid as its bank balance and its kinetic energy as its cash, the total capital of the stream-line (liquid tube) will be bank balance *plus* cash, and this is constant. If the liquid increases its velocity, or cash, it can only do so by drawing on its bank balance; and its cheques will only be honoured *up to this balance*, overdrafts not being permissible in the hydrodynamical bank. Similarly, if it reduces its velocity, or cash, it does so by paying into the bank and so increasing the balance there. As a perfect fluid has no viscosity, it may be said to have no "expenses": its capital will never diminish.

Water obeys the same law exactly, with one exception, and that is that $p + \frac{1}{2}\rho v^2$ is *not* constant, but is always diminishing slightly. Its viscosity causes the particles to rotate and so rub against one another. Energy is converted into the heat form, which is dissipated and so lost—to the water. In other words, water *has* "expenses" and so is living on its capital, which is therefore constantly decreasing.

In conclusion, *kinetic* energy cannot be directly transferred to a liquid. It must be paid into the bank as *potential* energy, which can be drawn on by the liquid in the usual manner.

A few examples will now be given illustrating the action of fluids in some very simple cases.

1. In the common scent spray, or the "atomiser," used in horticultural work, a powerful blast of air is caused to flow along the tube A B and just over the top of the tube B C, which is dipping into a liquid. Since the pressure in the air issuing from B is considerably less than the atmospheric

pressure, the liquid will rise from C to B, when it is "atomised," as shown in the sketch.

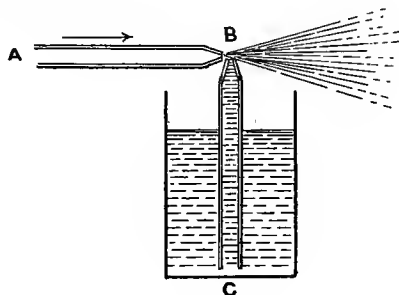


FIG. 3.

2. If water, under a high head, be flowing rapidly through a lead pipe, and a small hole be bored in this pipe, the water will not flow *out* by this hole, but, on the contrary, *air will flow in*. As in Venturi's experiment, if a small pipe be fixed to this hole, and the end of the pipe be dipped in liquid, this liquid will be sucked into the lead pipe.

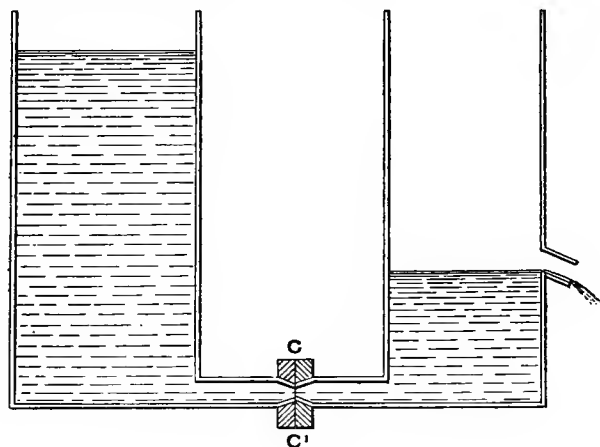


FIG. 4.

3. Another very pretty and well-known lecture table experiment, which used to be a favourite one of Lord Rayleigh, is to arrange two vessels of water, as in the sketch, the plates

CC' being simply placed together, but not fastened. When the water is flowing freely, the vessels can be separated at CC', when the water will spring across the gap just as if the tube were continuous.

4. The ordinary "injector" works, of course, on the same principle.

5. A very pretty experiment due to Mr W. Child may not be so well known. Two half "ping-pong" balls were mounted on a lath as anemometer cups, turning on the pivot, as shown in the sketch. If we now strike or poke one of the cups with a stick (in the direction of the plane of

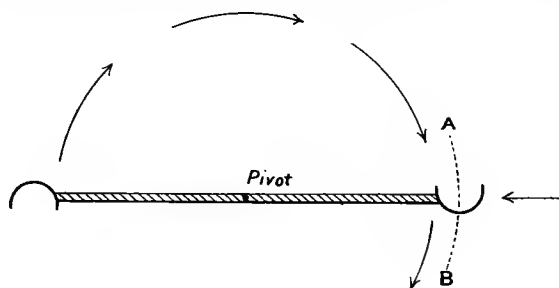


FIG. 5.

the paper) along the arrow, the cup will, of course, turn towards A. If, however, we cause a blast of air to strike it in the direction of the arrow, the cup will move *towards* B. In the one case the pressure is increased, whilst in the other it is decreased. In the same way, if the lath be suspended by a string instead of being fixed on a pivot, and then the lath be raised and lowered by means of the string, the whole arrangement will rotate in the direction of the circular arrows.

A very simple and pretty experiment showing the difference between the action of very fine dry sand, which does not *flow*, and that of water, was exhibited at the Royal Institution by C. E. S. Phillips in 1910. Take a small tube of about the diameter in the sketch and open at both ends. Cover the bottom with a cigarette paper, fastened on by an elastic band. The tube is then partially filled with fine dry sand and a piston is inserted on the top of the sand. As

the sand does not "flow," by carefully applying weight to the piston (as shown by arrows), the weight of a man can be suspended from the piston. It is hardly necessary to say

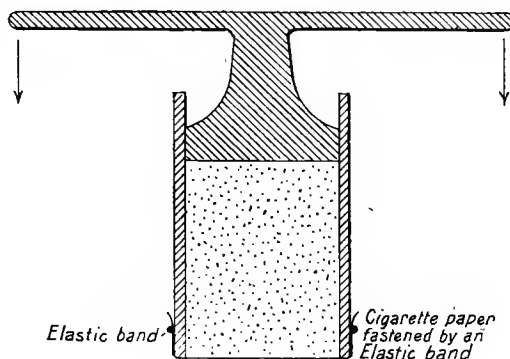


FIG. 6.

what would happen if water had been put into the tube instead of fine dry sand.

Very many more examples might be given, but the reader can supply these for himself: some special cases will also be referred to later.

SUMMARY

A liquid flows — can *only* flow — and *must* flow from a region of higher pressure to a region of lower pressure.

A liquid does *not*, under ordinary conditions, obey Newton's second law of motion. When it is stressed it does not move in a straight line, in any direction.

As the velocity of a liquid increases its pressure decreases.

A stream of liquid can only increase its velocity by converting some of its *potential* energy into *kinetic* energy.

Kinetic energy cannot be *directly* conveyed to a liquid. An energy transferred *must be potential*.

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 DUBUAT, *Principes d'Hydraulique*.

CHAPTER IV

RESISTANCE, CONSIDERED GENERALLY

HAVING explained how a liquid flows and the law regulating this flow, it will be well now to get a few clear ideas about liquid resistance. I propose to examine the subject from three points of view: (1) Newton's view; (2) that of the mathematicians; and (3) that commonly taught in the present day, and which I will call, for brevity, Dr Froude's view, since he appears to have been one of the first who taught it.

1. Newton, in his *Principia*, says: "In mediums void of all tenacity, the resistances made to bodies are in the duplicate ratio of the velocities"; also, at page 54: "And that part of the resistance which arises from the density of the fluid is, as I said, in the duplicate ratio of the velocity." He is even more precise, for he says, in another place, that it "cannot be less."

2. The mathematicians who treat of the stream-line theory have shown that in a "perfect" liquid of *infinite dimensions* a body moving would meet with *no resistance at all*.

These two statements appear, at first sight, as absolutely contradicting one another. The mathematicians must, of course, be right: granted the data, the result cannot well be in error; but "it is important to remember that the mathematical method as applied to physics must always be trustworthy, or untrustworthy, according to the trustworthiness of the data which are employed; that the most complete presentation of symbols and processes will only serve to enlarge the consequences of error hidden in the original premises, if such there be" (Langley, *Aerodynamics*).

It will be quite evident, from what has been said previously, that Newton and the mathematicians are speaking of different things. The latter only treat of flow "by obligation," whilst Newton refers to an ordinary liquid which is capable of "free" flow and consequently of "discontinuity." Both may be correct—and, indeed, both are. To reconcile Newton's view with that of the mathematicians it would appear to be necessary to somewhat amplify his formula and to say that R varies as $\rho V^2 \times F\left(\frac{1}{D}\right)$, where D = depth, and $F\left(\frac{1}{D}\right)$ is some indetermined inverse function of D which would become *zero* at some depth, depending on the shape of the body moving in the liquid, and which would remain *zero* for all greater depths.

For example, in an ocean of perfect liquid a body moving near the surface would experience a resistance varying as the *square of the velocity*. At greater depths this resistance would decrease; whilst at infinite depth (even with mathematical sharp edges) it would meet with *no resistance at all*.

This is in accordance with Lord Kelvin's views (quoted previously) when he says an oblate spheroid of 10 inches diameter and $\frac{1}{16}$ inch thickness would, if immersed in an inviscid water, meet with resistance at a less depth than 63 feet, but not beyond that depth of immersion.

3. Dr Froude's view is that, if the ocean were a "perfect" liquid, a fish or torpedo would meet with *no resistance at all*, and, once moving, would go on for ever.

Dr Fleming, who is one of the eminent men who appear to teach this, says, in his *Waves and Ripples*: ". . . If we could obtain a perfect fluid in practice, it would be found that an object *of any shape wholly submerged in the fluid* [italics added] could be moved about in any way without experiencing the least resistance." I have italicised "of any shape"; and the employment of the words "wholly submerged" shows that Dr Fleming admits the possibility of its *not* being wholly submerged: evidently he postulates a "free surface."

Naval architects apparently hold views which are most curious and incomprehensible. They would appear to be

rather like Jekyll and Hyde. When they speak of the forward motion of a ship, a fish-like body, or a torpedo, then they say the body *divides* the water and does not *push it forwards*. When they reach the stern of the ship, however—having presumably allowed the necessary quarter of an hour to elapse—and speak of the propeller, then they say the water is *driven backwards* by it. Apparently they would seem to believe that a spherical body *cannot* push water, whilst a circular flat plate *can*. If, however, the sphere be supposed to be gradually flattened and to pass through the orange and muffin shape to that of a flat plate, which would be the exact shape when the pushing would begin? Where does the law change, and why? It has always appeared extraordinary to me that Professor Rankine, who was one of the leading exponents of the stream-line theory, should also have been the author of the theory of the screw propeller, and to have stated that *all* propellers act by driving water backwards, and so causing the forward reaction to propel the ship.

It has been said that to understand an idea it is necessary to study its pedigree. What is the pedigree of this idea that a perfect fluid *could* offer *no* resistance? It would appear to be somewhat as follows. The stream-line theory was first propounded by certain mathematicians, who, for convenience, treated their "perfect" fluid as of infinite extent: they found that a body moving in this perfect fluid would not encounter any resistance. This point no one disputes.

In 1875 Dr Froude, when president of Section G at the British Association, selected for the subject of his masterly address the motion and resistance of liquids. He may be said to have, on this occasion, popularised the "stream-line" theory.

Dr Froude made no pretensions to being a mathematician, for he said the mathematics "are beyond my ken and my purpose," but he was obviously following the lead of the mathematicians.

We notice that, in the opening part of his address, he was *most* careful to speak of an "unlimited ocean of fluid"—an "infinite ocean of perfect fluid," and "a submerged body in the midst of an ocean of perfect fluid, *unlimited in every*

direction." All this was most proper and correct. As the address proceeds we see less and less insistence on the "infinity" of the ocean, until, towards the end, we find Dr Froude frankly stating: "I have shown that a submerged body, such as a fish or torpedo, travelling in a perfect fluid *would experience no resistance at all.*" This is, by no means, exactly what he had said previously, which was that in an *infinite ocean* of perfect fluid, etc. The two things are not the same at all, although at first sight they may appear to be so. The mathematicians had proved one, but the other appears to be a pure assumption, in support of which not a particle of evidence has ever been produced.

This statement of Dr Froude's appears to have been accepted without question, and is very commonly taught to this day.

The case may be put briefly thus:—

1. The flow causing "no resistance" *must, necessarily*, be an "electric flow."
2. An electric flow *necessitates* an *infinite velocity* of the fluid at every sharp edge.
3. This infinite velocity of the fluid *necessitates* an *infinite potential*; or, in other words,

$$P + \frac{1}{2}\rho v^2 = \text{infinite.}$$

4. As the immersion assumed is finite,

$$P + \frac{1}{2}\rho v^2 = \text{constant and finite.}$$

∴ The flow cannot be electric, and the assumption of "no resistance" is an unsound one.

When Dr Froude, in 1875, had satisfied himself that a fish or torpedo—why should naval architects lay such stress on the fish or torpedo?—moving in a perfect liquid would meet with no resistance, he, very naturally, supposed that all the resistance encountered by a body moving in a viscous liquid, like water, was *caused by the liquid friction*. This has led to other complications and errors; for, as he had found by experiment that the resistance of a body moving through water varied—more or less—as the *square of the velocity*, the

inference was that the resistance *due to friction* varied *in this same ratio*. As will be shown later, this is not correct, since it only varies *as the velocity*, as was pointed out by Newton more than 200 years ago.

If the resistance of a body moving in water were due to viscosity *only*, it would be natural to expect that the resistance would be the same whether the "fish or torpedo" were towed head first or tail first, which is by no means the case—a sailor knows that a spar towed with the butt foremost offers less resistance than if towed with the point first. To get over this new difficulty the term "eddy-making resistance" was introduced; and at present the accepted view appears to be that the resistance of a "fish or torpedo" is caused by fluid friction *plus* eddy-making resistance. As these appear to be the only two *possible* sources of resistance, no objection can be raised to this view, the only difference of opinion being on the point of *how much* is due to fluid friction and how much to "eddy-making resistance." Apparently, the ordinary method is to take the resistance due to the viscosity at a certain amount (found by experiment) *per square foot* of the wetted surface, and varying as the 1.83 power of the velocity, the remainder being put down to "eddy-making resistance." This is fairly simple, if not exactly scientific; but there are cases where even this does not agree very well with facts, so a further complication has been added, called the "augmented wetted surface." "This augmented surface was intended to represent the *plane* area, having the same resistance as the actual wetted surface, moving at the same speed. It might be termed 'the equivalent plane surface'" (Atherton and Mellanby's *Resistance and Power of Steamships*, 1903). This leaves the subject in a state which can only mildly be expressed as "perplexing." In practice, if a naval architect wants to know the resistance of a ship, he makes an exact model to scale and finds the resistance of this model *by experiment*. Knowing this, a very simple calculation, by Dr Froude's well-known laws of proportion, will give the resistance of the full-sized ship with an extraordinary degree of accuracy.

If the accepted view of the resistance of a ship be somewhat obscure, the resistance of a flat plate, moving broadside on to the water, is vastly more so. The whole theory applied to the "fish or torpedo" crumbles away like a child's house of cards.

Mr R. E. Froude, at Greenock in 1894, said: "In these questions I believe that we are treading on very difficult ground, but this much I imagine may be safely said in reply: in the cases here supposed [a propeller blade and the keel of a ship] the flatwise reaction is *not due to the friction of the water, nor in any sense proportional to it.*

"The resistance arises *directly out of the inertia of the fluid, in virtue of the establishment of a system of stream-lines of a totally different character from those of no resistance, which is theoretically proper to a frictionless fluid, and the presence of a certain frictional quality in the fluid—perhaps in a very moderate degree—makes the resisting system of stream-lines possible*" [italics added].

This "explanation" leaves one very little wiser than before. Certainly, if the author had a clear idea of what he meant, he appears to have been most careful to keep it to himself. From this *macédoine* of words, however, we can gather that—

(1) The resistance of a flat plate is *not* due to friction, but is *directly* due to the *inertia* of the liquid.

(2) This inertia resistance would *not be possible* but for the liquid having some—possibly very small—frictional quality.

Of these we may accept (1) as correct, but it will be shown later that (2) is not—Lord Kelvin's view, quoted previously, being in direct contradiction of (2). It would appear to be true that, if a perfect liquid existed, a screw propeller would work as well in it—if not better—than it does in water.

Mr R. E. Froude appears, at times, to almost regret the old theory of fluid resistance, for he says: "There are undoubtedly cases of fluid resistance in which the old theory, or something like it, is still applicable," thus almost echoing Lord Rayleigh's view, quoted previously.

The old theory may have been bad; may have been—

almost undoubtedly *was*—wrong; but it had, at least, one merit: it was intelligible, which is perhaps more than can be said of the present accepted view.

We therefore come back to Newton's view—which is the same as Lord Kelvin's—that “in mediums void of all tenacity the resistances made to bodies are in the duplicate ratio of the velocities.”

In the next chapter the subject of viscosity and fluid friction will be gone into, and we will see how this friction modifies the results. Subsequently the question of resistance will be gone into in more detail and comparison made between theory and experiment.

SUMMARY

The resistance of a body moving in a “perfect” or inviscid liquid would vary as the density of the liquid and as the square of the velocity of the body, and would continually diminish as the depth of immersion; would vanish at a certain finite depth, and at all greater depths the resistance would remain *zero*. If, however, the body be supposed to have mathematical sharp edges, then the immersion must be to an infinite depth for the resistance to become *zero*.

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CHAPTER V

VISCOSITY AND FLUID FRICTION

BESIDES the resistance which is due to the density of the liquid, there is another which is due to the stickiness or "treaciness" of the liquid. Newton refers to it as the "want of lubricity," and he says: "The resistance arising from the want of lubricity in the parts of the fluid is, *cæteris paribus*, proportional to the velocity with which the parts of the fluid are *separated from each other*" [italics added]. It is clear that Newton considered this resistance as a stress in the liquid to resist shearing. Text-books on hydraulics do not, as a rule, devote very much space to liquid friction, but it may be advisable to recapitulate their statements thereon; and probably the very best description is that found in Perry's *Applied Mechanics*.

He says: "Water flowing in a certain pipe at the velocities 1, 2, 3, etc., inches per second, the friction is proportional to the numbers 1, 2, 3, etc.; whereas at the velocities 1, 2, 3, etc., yards per second the friction is proportional to the numbers 1, 4, 9, etc. At small velocities three times the speed means three times the friction; whereas at great velocities, such as those of ships, three times the speed means nine, or more, times the friction."

In this paragraph it is necessary to understand what is the exact meaning of the word "friction." If, as is probable, Perry meant it as equivalent to "resistance," then it is indisputable. If it be employed as *resistance due to viscosity*, then it cannot be accepted as a correct statement of the facts. In the second case, the total resistance is *that due to viscosity*

plus "eddy-making resistance." Because the total resistance has increased as 1, 4, 9, etc. (which is a fact), it does not follow, as a consequence, that *therefore* the resistance due to viscosity has increased in this ratio. If the viscosity were the cause of the eddy-making resistance, then the statement might be true; the exact reverse, is, however, the case: viscosity tends to *prevent* eddy-making.

To continue: "At constant temperatures below a certain critical speed, I found that the friction was proportional to the velocity, so that μ could be found. At the critical speed I found there was a sudden change in the law [italics added], and above that speed the friction [? total resistance] is proportional to a higher power of the speed than 1. We know that above the critical speed the plane motion, which I described above, would become unstable and eddies would be formed. . . ."

"At speeds below the critical, I measured μ at many different temperatures, and noted the rapid decrease of it as the temperature increased."

If we assume, as before, that the word "friction" has been employed in two rather different senses, we may put Perry's statement somewhat as follows: The resistance due to viscosity varies as the velocity, but that at a "critical speed" the motion of the liquid becomes unstable, eddies are formed, and a new resistance—which is an inertia resistance¹—may be said to be "switched in" suddenly. It is well to remember, also, that the resistance due to viscosity decreases rapidly as the temperature increases.

Professor Perry's summary is very much to the point, and is as follows:—

"1. The force of friction very much depends on the velocity and is indefinitely small when the speed is very low.

"2. The force of friction does not depend on the pressure.

"3. The force of friction is proportional to the area of the wetted surface.

¹ Inertia resistance is that resistance which is due to the continuous communication of momentum to the liquid surrounding the body. Mr Lanchester speaks of it as "dynamic resistance."

"4. The force of friction at moderate speeds does not much depend on the nature of the wetted surface."

In all these cases it is plain that by "friction" is meant the *resistance due to viscosity*. It is obvious that the friction of mercury in a glass tube is very small; but it does not follow that the viscosity of the mercury is also small. Rules Nos. 3 and 4 do not apply here, for there is no wetting of the glass at all; No. 4 will be referred to again later, for the statement requires qualification.

From what Professor Perry has said we would infer that the resistance is composed of two terms, one of which varies *as the velocity*, whilst the other varies *as the square of the velocity*.

In 1884 Professor Osborne Reynolds said at the Royal Institution: "In rivers, and all pipes of sensible size, experience has shown that the resistance increased as the *square of the velocity*, whereas in very small pipes, such as represent the smaller veins of animals, Poiseuille has proved the resistance increased *as the velocity*."

"Now, since the resistance would be as the square of the velocity with sinuous motion, and as the velocity, if direct, it seemed that this discrepancy could be accounted for if the motion could be shown to *become unstable* for a sufficiently large velocity."

Osborne Reynolds also found experimentally that the eddies were formed *suddenly* at a critical velocity when the law of resistance changed.

Stokes said: "In Coulomb's experiments it appeared that the resistance was composed of two terms, one involving the first power and the other the square of the velocity."

In 1808 Dr Young wrote (*Phil. Trans.*): "I began by examining the velocities of water discharged through pipes of a given diameter, with different degrees of pressure; and I found that the friction *could not be represented by any single power of the velocity*, although it frequently approached to the proportion of that power, of which the exponent is 1·8, but that it appeared to consist of two parts, the one varying simply as the velocity, the other as the square. The proportion of

these parts to each other must, however, be considered as different in pipes of different diameters, the first being less perceptible in very large pipes, or in rivers, but becoming greater than the second in very small tubes; while the second also becomes greater, for each given portion of the internal surface of the pipe, as the diameter is diminished."

The latter part of this last paragraph is, perhaps, a little obscure, but there can be no doubt of what Dr Young meant, for he gives the formula as

$$f = a \frac{l}{d} v^2 + 2c \frac{l}{d} v,$$

which may be expressed

$$f = \frac{l}{d} (av^2 + 2cv),$$

where l =length of pipe, d =diameter, whilst a and c are constants.

Lord Kelvin's views are similar to these, for in "Thomson and Tait" we find under the head of friction:

"340. According to the approximate knowledge which we have from experiment, these forces are independent of the velocities when due to the friction of solids, but are *simply proportional to the velocities* when due to *fluid viscosity, directly* [*italics added*], or to electric or magnetic disturbances, with corrections depending on varying temperature and on the varying conditions of the system. In consequence of the last-mentioned cause, the resistance of a *real liquid* (which is always more or less viscous) against a body moving rapidly enough through it to leave a great deal of irregular motion, in the shape of 'eddies,' in its wake, seems, when the motion of the solid has been kept long enough uniform, to be nearly in proportion to the square of the velocity; although, as Stokes has shown, at the lowest speeds the resistance is probably in simple proportion to the velocity, and for all speeds, after long enough time of one speed, may, it is probable, be approximately expressed as the *sum of two terms*, one *simply as the velocity*, and the other as the *square of the velocity*. If a solid is started from rest in an incom-

pressible fluid, the *initial law of resistance* is no doubt simply *proportional to velocity* (however great, if suddenly given), until, by the gradual growth of eddies, the resistance is increased gradually till it comes to fulfil Stokes' law."

All this evidence—and more might have been added—confirms the view that the resistance *due to viscosity* varies *as the velocity*, and that the total resistance experienced by a body moving in a liquid may be expressed by the simple formula

$$R = AV + BV^2,$$

where A and B are constants.

In the face of all this authority it is certainly "perplexing" to read in the Bluebook on *Aeronautics* over the signature of F. W. Lanchester:

"Notes on the Resistance of Planes in Normal and Tangential Presentation, and on the Resistance of Ichthyoid Bodies.

"The law of the pressure reaction as a function of velocity is approximately the same whatever the form of the immersed body, namely, $R \propto V^2$."

This would appear fairly clear, even though it may not agree too closely with facts; but later on we see:

"The V-squared law is also subject to error on account of the viscosity of the fluid. In this case the *error becomes greater the less the velocity*. For very low velocities, as shown by Stokes and more recently by Allen, it *breaks down altogether*. For low velocities the index becomes less than 2, and there appear to be two distinct stages or ranges of velocity during which the index approximates to 1.5 (Allen) and to 1 (Stokes) respectively."

This should be perplexing enough to satisfy anybody. We have a law which is

1. *Approximately, $R \propto V^2$.*
2. Which is "subject to error," on account of the viscosity.
3. The error "becomes greater the less the velocity," and
4. At very low velocities it "breaks down altogether."

The law appears unfortunate; for not only does it not hold good for low velocities, but it appears to be equally

unhappy for high velocities; for "as soon as any considerable variation in the velocity is contemplated it is necessary (as is well known to naval architects) to make a correction in the coefficients." Certainly, if one allows oneself sufficient latitude in the change of the coefficients, one can straighten out almost any law. In the last resort one can always blame viscosity!

* What would be thought of a law of gravity, say, which was approximately $G\infty\frac{1}{D^2}$, but which was "subject to error" for some cause or other, which broke down utterly at small distances, and which had to be corrected by different coefficients for planetary or stellar distances?

It has been pointed out that on reaching a "critical speed" a liquid becomes unstable and *suddenly* breaks into eddies, or, as Osborne Reynolds calls it, "sinuous motion." Why is this?

Dr Hele-Shaw says these eddies are produced—in some way which he does not attempt to explain—*by viscosity*. That eddies *can* be produced by viscosity is undoubted; but that they are ordinarily so produced does not appear correct.

Professor Osborne Reynolds in 1884 (Royal Institution) compared the flow of a stream of water to the movement of a body of troops, and the laws of hydrodynamics to a drill book. He then continued: "Suppose this science [of War] proceeds on the assumption that the discipline of the troops is perfect, and hence takes no account of such moral effects as may be produced by the presence of an enemy.

"Such a theory would stand in the same relation to the movement of troops as that of hydrodynamics does in the movements of water. For although only the disciplined motion is recognised in military tactics, troops have another manner of motion when anything disturbs their order. All this is precisely how it is with water; it will move in a perfectly direct disciplined manner under some circumstances, while under others it becomes a mass of eddies and cross streams which may well be likened to the motion of a whirling, struggling mob where each individual particle is obstructing the others.

"Nor does the analogy end here: the circumstances which determine whether the motion of troops shall be a march or a scramble are closely analogous to those which determine whether the motion of water shall be direct or 'sinuous.'

"In both cases there is a certain influence necessary for order: with troops it is discipline; with water it is *viscosity* or *treachiness*.

"The better the discipline of the troops, or more treachly the fluid, the less likely is steady motion to be disturbed under any circumstances. On the other hand, speed and size are in both cases influences conducive to unsteadiness. The larger the army, and the more rapid the evolutions, the greater the chance of disorder. So with fluids, the larger the channel, and the greater the velocity, the more the *chance* of eddies.

"With troops some evolutions are much more difficult to effect with steadiness than others, and some evolutions which would be perfectly safe on parade would be sheer madness in the presence of an enemy. So it is with water."

This very excellent analogy only lacks one thing, and that is the "enemy" of the water. It cannot be accepted that eddies are produced by "*chance*"—there must be some definite *cause*, and that appears to be undoubtedly the differences of pressure in different parts of the liquid.

Osborne Reynolds uses the expression "steady motion" as such movement of the stream-line which has not broken, or is not breaking into eddies, and "sinuous motion" when the stream-line is breaking, or has broken into eddies. He shows that viscosity is not only not the *cause* of eddies, but that it is the great restraining influence in *preventing them*. He also adds: "The effect of these influences is subject to one perfectly definite law, which is that a particular evolution becomes unstable for a definite value of viscosity divided by the product of the velocity and space. This law explains a vast number of phenomena which have hitherto appeared paradoxical. One general conclusion is, that with sufficiently slow motion all manners of motion are stable."

To prevent any misunderstanding as to what he means

by a stream, he says: "Solid walls are not necessary to form a stream; the jet from a fire-hose, the falls of Niagara, are streams bounded by a free surface. A river is a stream half bounded by a solid surface." He further gives the "circumstances conducive to direct or steady motion" as—

"1. Viscosity, or fluid friction which continually destroys disturbances (treacle is steadier than water).

"2. A free surface.

"3. Converging solid boundaries.

"4. Curvature with the velocity greatest on the outside."

"Those conducive to sinuous or unsteady motion are—

"5. Particular variation of velocity across the stream, as when a stream flows through still water.

"6. Solid boundary walls.

"7. Diverging solid boundaries.

"8. Curvature, with the velocity greatest on the inside."

From all the foregoing we see that the less the viscosity the more difficult it is to *prevent* a liquid from breaking into eddies.

Professor Perry (*Applied Mechanics*) also says: "A very frictionless fluid is very unstable."

Lord Kelvin's views are, as we should expect, clear and definite (*Phil. Mag.*, 1887): "It seems probable, almost certain indeed, that analysis similar to that of §§ 38 and 39 will demonstrate that the steady motion is stable for any viscosity, however small; and that the practical unsteadiness pointed out by Stokes forty-four years ago, and so admirably investigated experimentally five or six years ago by Osborne Reynolds, is to be explained by limits of stability *becoming narrower and narrower the smaller is the viscosity*" [italics added].

The natural deduction from all this would appear to be that if the viscosity were eliminated, or became *zero*—as in the imaginary perfect liquid—that the fluid would be perfectly unstable, and that it would be impossible to move it in any manner "conducive to sinuous or unsteady motion," without it breaking into eddies; and yet some authors would appear to wish the student to believe that though "a *very* frictionless liquid is *very unstable*," an *absolutely frictionless* liquid is

perfectly stable. Great faith is required to believe this: more especially as not a scrap of evidence is produced in support of it.

Helmholtz is very clear on this point (*Phil. Mag.*, xliii.): "Such great differences between what actually takes place and the deductions from the theoretical analysis hitherto accepted must cause physicists to regard the hydrodynamical equations as a practically very imperfect approximation to the truth. The cause of this discrepancy *might be supposed to lie in the internal friction of the liquid*, although the divers strange and saltatory irregularities which everyone has encountered who has experimented upon the motions of fluids *can in no wise be accounted for by the continuous and uniform action of the friction.*"

"The investigation of the case where *periodical motions* result from a *continuous air current*, as, for instance, in organ pipes, convinced me that such an action could only arise by a *discontinuous motion of the air*, or at least by an *approximately discontinuous* one. Hence I was led to the discovery of a condition . . ." involving what he calls "surfaces of separation."

"The moment the pressure passes zero and commences to become negative, *a discontinuous change in the density* takes place: *the fluid is broken asunder.*"¹

These remarks should be pondered over deeply, for when the reader has thoroughly grasped Helmholtz's meaning, he will be well on the road towards understanding the leading features of liquid resistance.

SUMMARY

Fluid friction is caused by a stress set up in a liquid to resist shearing.

It increases *as the velocity only*.

It is not affected by pressure.²

¹ Avanzini appears to have known this more than a hundred years ago.

² This is only *approximately* correct. "It has been shown by Warburg and Sachs, and also by Röntgen, that water at ordinary temperatures becomes more mobile when subjected to pressure: in other words, its viscosity is *lowered by pressure*" (T. E. Thorpe, Roy. Inst., 1898).

At a "critical speed" the liquid breaks suddenly into eddies.

The resistance of a body moving in a viscous liquid can therefore be expressed by

$$R = AV + BV^2,$$

where AV is the resistance due to viscosity, and BV^2 that due to eddy-making.

Viscosity is not in general the *cause* of eddies; it actually tends to prevent their formation and to stop them when formed.

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CHAPTER VI

A VISCOUS FLUID FLOWING "BY OBLIGATION"— DISCONTINUITY

IN Chapter I. it was stated that the flow of a viscous liquid should not be *very* different from that of a "perfect" one *under similar conditions*. If this is true, it follows naturally that if a viscous liquid be caused to flow "by obligation," it should behave almost exactly as the mathematicians say their liquid would—the motion being subject to some small variation caused by the viscosity.

Dr Hele-Shaw has, by a very beautiful piece of apparatus, actually caused a viscous liquid to flow "by obligation," and has thus thoroughly confirmed experimentally what the mathematicians had predicted theoretically. His description of the experiment is as follows:—

"If we take two sheets of glass and bring them nearly close together, leaving only a space the thickness of a thin card or piece of paper, and then by suitable means cause the liquid to flow *under pressure* [italics added] between them, the very property of viscosity, which, as before noted, is the *cause of the eddying motion* in large bodies of water, in the present case *greatly limits the freedom of motion of the fluid* between the two sheets of glass, and thus *prevents* not only the eddying motion, but also *counteracts the effect of inertia*."

In examining this statement, and before proceeding further, it may be pointed out that (1) this is a case of the closed tank referred to in Chapter II.; the liquid is under pressure, and the flow (within limits) will be an "obligation" flow and not a "free" flow. (2) As shown in the last chapter, it cannot be

accepted as a fact that viscosity is the *cause* (or *chief* cause, for it may in special circumstances be the cause) of eddying motion in "large bodies" of water: it acts in exactly the opposite manner, and *prevents*, or tends to prevent, eddies. Dr Hele-Shaw, of course, actually employs it *for this very purpose*. As previously remarked, since water is not *sufficiently viscous*, he makes use of glycerine. It is hardly reasonable to expect anyone to believe (without proof) that viscosity *causes* eddies in *large* bodies of liquid, whilst it at the same time

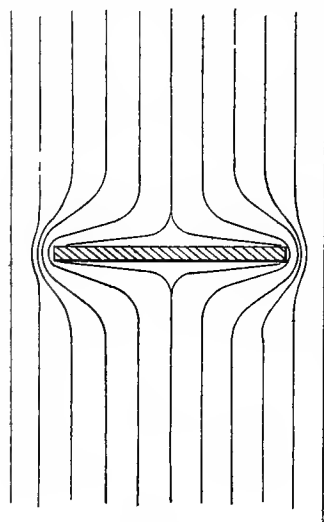


FIG. 7.

prevents their formation in *small* bodies of liquid. (3) The viscosity *counteracts* the effect of *inertia* by preventing eddies, and so (practically) eliminating the mass of the liquid.

"If we now, by suitable means, allow distinguishing bands of coloured liquid to take part in the general flow, we are able to imitate exactly the conditions in the diagrams" [of flow of the mathematicians' fluid].

The imitation is most perfect, whether the flow be past an obstacle, through a narrow gap, "source and sink" flow, etc., etc. To give one example only, the flow past the obstacle is as shown in the sketch (fig. 7), and not in the least in the usual manner of a viscous liquid, like water. The velocity is, of course, kept very low; the pressure, also, is sufficient to cause the liquid to "turn the corners" properly.

If, however, the flow be allowed a little freedom, a very different result is produced. This Dr Hele-Shaw effects by setting the plates of glass a little further apart, when, as he says, "the water *refuses to flow round to the back*, and spreads on either side," as in the sketch (fig. 8).

This flow, which is no longer "by obligation," is what

Mr R. E. Froude calls a "potential cleavage" flow; it might also be called the Helmholtz-Kirchhoff" flow, as they were the first to study it. It is also referred to by Lord Rayleigh in the paper previously quoted from.

In this flow it will be observed that there has been a permanent cleavage of the liquid reaching A B, and that the liquid then flows right and left past the stagnant pool A B C D. Since the liquid is flowing along A C and B D at a higher velocity than it had before reaching A B, and since the liquid in the pond is *at rest*, if what has been stated previously as regards the *law of flow* be correct, the pressure in the pond *must be greater* than in the streams along A C and B D. This, of course, follows from the law of flow referred to in Chapter III., and which was expressed in "algebraical shorthand" as

$$P_0 = p + \frac{1}{2}\rho v^2$$

or

$$p = P_0 - \frac{1}{2}\rho v^2.$$

The flow in the case under consideration is no longer an "obligation" flow, and

the mass of the liquid has not been "practically eliminated"—hence ρ , the density, is important. The pressure in the pond is here expressed as P_0 , which is the pressure in the imaginary stream-line when $v=0$. In the streams along A C and B D the pressure is $P_0 - \frac{1}{2}\rho v^2$, the value of which can be arrived at when we know P_0 and v . The two pressures on the opposite sides of the "surfaces of discontinuity" are therefore as $P_0 : P_0 - \frac{1}{2}\rho v^2$, *i.e.* the pressure on the pond side is greater than that in the moving liquid.

If what was said in Chapter III. is true, that a liquid *must* flow from any region of higher pressure to any region of lower

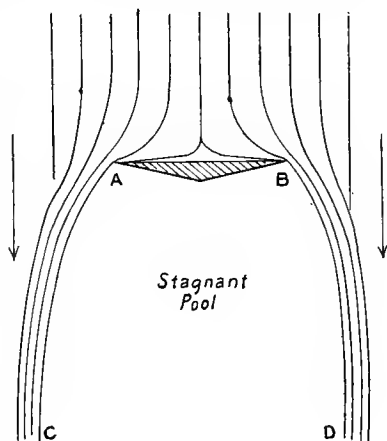


FIG. 8.

pressure, why is there no flow *from the pond into the flowing stream*? What is preventing this flow?

The reason is that the liquid has been "torn asunder," and that the tear extends along AC and BD. Along AC and BD there are "surfaces of discontinuity," or free surfaces. As the free surface is capable of sustaining tension, although it is "stressed" the film does not break, and so allow the liquid to flow. The case is similar to that of a soap-bubble, where the film is retaining the air inside, which is *under greater pressure* than the atmospheric. As long as the "excess of pressure" on the pond side of AC and BD is not too great, the flow continues; but if this excess be increased, which



FIG. 9.

can easily be done by increasing the velocity and so reducing the pressure in the flowing stream, the breaking point is soon reached.

If the velocity of flow be slightly increased the result is rather curious: the lines AC and BD gradually approach one another, as in the sketch. The pressure is still not sufficient to break the film: a little more velocity, the film breaks and the liquid flows *from the pond into the stream*, as shown in the sketch (fig. 9). This is exactly what we should have expected for the reason previously given.

It may be asked, why should the stream-lines AC and BD gradually *approach* one another? I am not aware that any explanation has ever been given; so I will venture the following, which will serve as a working hypothesis until a better one is offered. The stream-lines along AC and BD act like the well-known Sprengel pump—in the manner pointed out by Venturi many years ago. We thus have—

1. The liquid in the pond evaporates *through the film into the void space*.
2. The streams AC and BD absorb this vapour and so increase the evaporation.

3. This action continuing, the pond gets smaller, and, *ergo*, A C and B D approach one another, and eventually meet.

All Dr Hele-Shaw's beautiful experiments show that a viscous liquid, when made to flow "by obligation," will move *almost* exactly as the mathematicians' fluid would *under similar conditions*.

I have dwelt very long over this experiment, and the reason is that I think we are here assisting at the birth of a vortex. This is, I believe, the first stage in the formation of vortices: the diagram given by Lord Kelvin in his paper on "Coreless Vortices" I consider to be the second stage. As the resistance of bodies moving in liquids is chiefly caused by the formation of vortices, it is well to think carefully about this experiment.

This "potential cleavage" flow was imagined, as previously stated, by Helmholtz and Kirchhoff as a possible flow of a perfect liquid past a circular lamina.

There are certain grave objections to this form of flow. Some of these have already been referred to; but there is another pointed out by Lord Kelvin. In his paper on "Coreless Vortices," he says, about the fluid, when it passes the sphere: "It might be thought that the result of this collision is a 'vortex-sheet' which, in virtue of its instability, gets drawn out and mixed up indefinitely, and is carried away by the fluid further and further from the globe. A definite amount of kinetic energy would thus be *practically annulled* in a manner which I hope to explain in an early communication to the Royal Society of Edinburgh.

"But it is impossible, *either in our ideal inviscid incompressible fluid, or in a real fluid*, such as water or air, to form a vortex-sheet, that is to say, an interface of finite slip, by any natural action."

Another objection is that as the pressure on the face of the lamina (the side in presentation) must be *less* than the hydrostatic pressure, there is only one small spot in the centre where the pressure attains the H.P.; at all other parts it is less than this; and as the pressure on the

rear of the lamina *must be the hydrostatic pressure*, called in the formula of flow P_0 , it follows that the lamina would propel itself and we should have a fine example of perpetual motion.

A third objection is that the action could not be reversed. This argument will appeal to engineers; for one can hardly imagine a lamina moving through a liquid and dragging an indefinite (not to say infinite) body of water behind it like a tail. Just imagine what force it would require to start this tail moving, without entering into the mechanical difficulties of keeping the particles of the tail together.

Possibly the severest condemnation of this flow was that made by Lord Kelvin in *Nature* in 1894: "The assumption to which I object as being inconsistent with hydrodynamics, and very far from any approximation to the truth for an inviscid incompressible fluid in any circumstances, and utterly at variance with observation of discs or blades (as oar blades) caused to move through water, is that, starting from the edge, as represented by the two continuous curves in the diagram, and extending indefinitely rearwards, there is a 'surface of discontinuity,' on the outside of which the water flows relatively to the disc, with velocity V , and on the inside of which there is a rearless mass of 'dead water.'

"The supposed constancy of the velocity outside of the supposed surface of discontinuity entails for the inside a constant pressure, and therefore quiescence relatively to the disc, and rearlessness of the 'dead water.' How could such a state of motion be produced? and what is it in respect of its rear? are questions which I may suggest to the teachers of the doctrine, but which, happily, not going in for an examination in hydrokinetics, I need not try to answer."

When all has been said against it, one must admit that Dr Hele-Shaw has actually shown, experimentally, that such a flow is possible *in a liquid of practically two dimensions and at very low velocities*. It is, at least, *imaginable* in a non-viscous liquid of three dimensions, with the *lamina* at rest and the

liquid flowing at an exceedingly low velocity, provided that—

1. The liquid has a free surface, or
2. The boundaries of the liquid are not fixed, but extensible,

or

3. The liquid is compressible—any one of these conditions is sufficient—and

4. Provided that the surface of the inviscid liquid is capable of sustaining surface tension.

I do not know the original paper by Helmholtz and Kirchhoff, but I rather fancy that the liquid is assumed as *incompressible* and of infinite dimensions, *i.e.* that it has *not* a free surface and that its boundary walls are *fixed*.

If this Helmholtz-Kirchhoff theory of flow has been discussed at such length, it is only because, in consequence of the eminence of the authors, it is so commonly referred to by writers; it has thus acquired a notoriety which I can only think is far beyond its merits.

It is generally admitted that it is *not* a "no-resistance" flow: it therefore only tends to support an argument that flat plates obey different laws to round or "fish-like" bodies, which we cannot admit. This theory may appeal to mathematicians, but it is hardly likely to do so to engineers.

DISCONTINUITY

In the course of this chapter the word "discontinuity" has been used, and it has been employed in the sense of the particles of the liquid having been "torn asunder." It has been objected to me, and, doubtless, may be again, that this "discontinuity" is not *physical* discontinuity, but *kinetic*: in other words, that there is no *separation* between the layers of liquid, but that they are moving at different speeds—in the case here referred to one is actually at rest. This appears to be rather a case for the objectors to show that such *kinetic* discontinuity is *possible* without the production of a free surface. It is about as easy to imagine that in a bar of iron one layer of particles were red-hot and the next layer at the

temperature of liquid hydrogen. Professor Lamb always speaks of a "surface of discontinuity, or, what is the same thing, a free surface." Sir George Stokes spoke of discontinuity or "rift." Helmholtz refers to "surfaces of separation." There is no doubt about what Lord Kelvin meant. In using the term "discontinuity," I always mean to imply that the fluid has been torn asunder so as to produce actual *physical* discontinuity. This physical "cracking" of a fluid is very common in everyday life, and explains many things. The spark of a Rühmkorff coil "cracks" the air, just as it will crack a sheet of glass. Lightning similarly cracks the air, and the resulting noise is thunder.

When a gun or a rifle is fired the hot gases issuing very rapidly "crack" the air: even when you crack a whip you do the same thing. The loudness of the report depends, very largely, on the suddenness with which the crack is made. To prevent any possibility of being misunderstood, it may be necessary to point out that it is not the physical crack of the air which *directly* causes the sound: this is produced by the rush of air which takes place afterwards.

There are many cases where such "cracking" of the air would hardly be suspected—*e.g.* organ pipes, whistles, etc.; but in these cases it may be that physical separation is never *complete*, but that there is only high rarefaction.

"It is probable that much of the foam seen near the sides and in the wake of a steamer going at a high speed through glassy-calm water is due to 'vacuum' behind edges and roughnesses causing dissolved air to be extracted from the water" (Lord Kelvin, *Nature*, 1894).

SUMMARY

A viscous liquid, when made to flow "by obligation," does so in a similar manner to that laid down by the mathematicians for their "perfect" liquid.

The form of flow imagined by Helmholtz and Kirchhoff, and called by Mr R. E. Froude the "potential cleavage" flow, is not a possible one, except under such very narrow

conditions as are not usually found in Nature, *because it is essentially unstable*. The liquid flowing past a lamina does, undoubtedly, *commence* flowing in this manner; but the flow *immediately breaks down* and eddies are formed. This would be equally true in a "perfect" liquid, or a viscous one, like water.

"Discontinuity" is a physical separation of the liquid, which then has a free surface which is capable of sustaining tension.

REFERENCES

Dr HELE-SHAW, "On the Motion of a Perfect Liquid" (R. Inst., 1899).

Lord KELVIN, "Coreless Vortices," etc., *Phil. Mag.*, 1887.

„ „ *Nature*, 1894.

CHAPTER VII

VISCOUS LIQUID NOT FLOWING "BY OBLIGATION," BUT FOLLOWING STREAM-LINES GIVEN IN HYDRO- DYNAMICAL TEXT-BOOKS

IN the last chapter it was pointed out that a viscous liquid, if made to flow "by obligation," would move in stream-

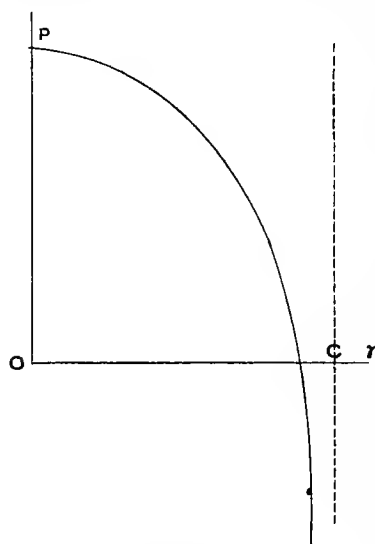


FIG. 10.

lines similar to those of the mathematicians' perfect liquid. There are cases where it does so, *even when it has a free surface*, as there are cases where it does *not*.

When water is flowing to meet a body immersed in it, the flow before arriving at the body is a perfectly "continuous flow," and so should, of course, follow the orthodox hydrodynamical stream-lines. It is notorious that it does so; and these stream-lines have been photographed by M. Marey and others, who have employed various devices for

making the stream-lines visible. It should also *follow the same laws* as they do.

It is, I believe, generally accepted that if a thin lamina—or even any other body—moves in a perfect liquid, the pressure

on the anterior face will be *less* than it would be were the lamina at rest.

In reference to a circular lamina exposed to an infinite perfect fluid moving past it (fig. 10), Professor Lamb says: "The velocity at a distance r from the centre of the disc ($\text{rad} = c$) will be

$$\frac{2}{\pi} \cdot \frac{r}{\sqrt{(c^2 - r^2)}} U,$$

when U is the general velocity of the stream.

"Consequently the pressure

$$p = p_0 - \frac{2}{\pi^2} \cdot \frac{r^2}{c^2 - r^2} \rho U^2$$

(absolute units) when p_0 is what the pressure would be if the fluid were at rest." The pressure curve would be something like that in the sketch (fig. 10).

It will be seen at once that $p = p_0$ when $r = 0$, or at the centre of the lamina: on all other parts of the disc the pressure is less than p_0 , which is the hydrostatic pressure. It is also well to remember that the pressure at the centre of a lamina—or other body—is always the hydrostatic pressure, for the liquid at the "dividing point" is at rest, referred to the body.

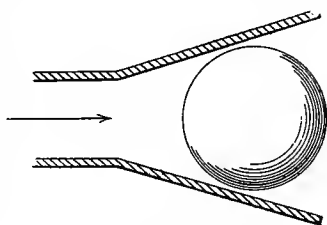


FIG. 11.

Is this the same for water? From what has been previously said, since all the water in front of the lamina is *flowing*, it *must* be under lower pressure than if it were at rest: it can be shown, experimentally, that such is the case.

Some years ago a company had on sale a nozzle for fire purposes called a "ball-nozzle." This nozzle was cone-shaped (as in the sketch, fig. 11), and inside this cone was a hollow ebonite ball, which was quite loose. When the water was turned on to the hose from the main, the ball remained firmly fixed in the mouth of the cone, and remained fixed *as long as the water was flowing*. This is a practical proof that the

pressure of the *water* against the ball was *less than the atmospheric* (!), though the "head" on the main may have been sufficient to produce a pressure of 30 or 40 lbs. per square inch.

What is true for water is equally true for air. If a small cone be made of tin (about the size of the sketch, fig. 12), and if a tube of about the size of a small quill be fitted into the apex of the cone, on blowing hard through this, a ball of light wood can be kept suspended, against gravity, in the cone. If, in fact, the ball be placed in the palm of the hand and the cone (with a stream of air passing through it) be made to approach the ball, the latter can be caused to *jump off the*

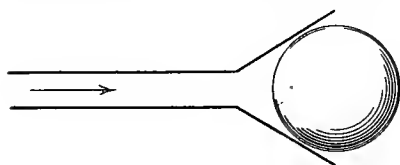


FIG. 12.

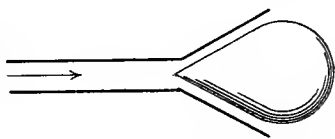


FIG. 13.

hand into the cone. This experiment can be made by anyone with a *minimum* of trouble.

To prevent any possible misunderstanding, when it is stated that the ball is "fixed" in the mouth of the cone, it is not pretended that the ball *ever touches the cone*: it does not. The pressure of the atmosphere *forces it in*; but it will be evident that, if the ball gets too near the cone, the supply of water will be reduced: the flow of the water between the cone and the ball *decreasing*, the pressure behind will *increase*, and the ball will move outwards again. There is probably a very small oscillation of the ball in the mouth of the cone.

It is interesting to note that the same result is not arrived at if the ball is pointed (towards the cone), *i.e.* if it is a cone with a hemispherical base. In such a case the form is highly unstable and there is what Mr Bairstow (Bluebook, *Aeronautics*, 1910-11) calls a "negative righting moment," which forces the point against the side of the cone and so spoils the effect. This is an argument against the supposed advantage of a body having a fine "entry."

It may be argued that these are "round" bodies and that what is true for them may not be true for "flat" bodies. Quite so. Almost exactly the same experiment may be made with a flat body. If a small glass tube be fixed to a brass plate (fig. 14), on blowing through the tube and bringing the glass plate near to a sheet of paper, the latter can be lifted up and kept suspended as long as the blowing is continued.

How is the pressure distributed?

More than a century ago the Chevalier Dubuat (Colonel of the French Royal Engineers) published his *Principes*

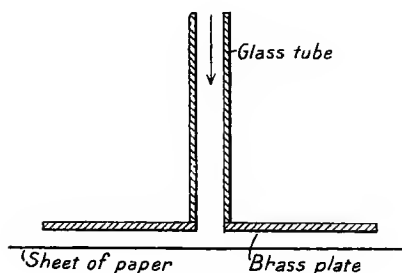


FIG. 14.

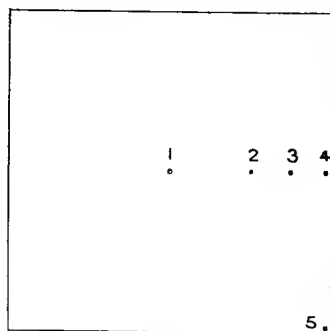


FIG. 15.

d'Hydraulique, where he says: "Quand l'eau coule le long d'une surface en vertu d'une charge supérieure, la hauteur due à la pression, qu'éprouve cette surface est égale à celle qui avait lieu, si le fluide était en repos, moins la hauteur due à la vitesse moyenne réelle du fluide dans le sens parallèle à la surface." From this he deduced: "D'après, ce principe, la pression occasionnée par le choc contre une surface doit diminuer du centre à la circonférence."

He examined this experimentally by exposing a submerged plate (fig. 15)—in reality a very thin box—to a moving stream, and measuring the actual pressures at different parts of the surface by means of open holes and the well-known Pitot's tubes. This is, of course, far better than taking the whole resistance of the plate, which is really only

the difference between the pressures on the front and on the back, and which teaches nothing about the distribution of the pressures on the anterior surface.

He was prepared, as we have seen, to find a greater pressure at the centre of the plate than at the edges; but when he made the experiment he was surprised to find that a *negative* pressure was recorded at the edges. He says: "On remarque d'abord dans ces expériences, un effet *bien singulier*, et que nous ne pouvions pas prévoir: c'est que la pression, qui diminue du centre de la surface vers les bords, devient nulle à une certaine distance, et ensuite négative au bord même."

His figures are—

(The measurements are old French ones, but are useful for comparison *one with another*.)

No. 1. Centre hole of the box
open, the other four
closed: the height
corresponding to
the pressure at this
hole was . . . + 32·8 lignes.

No. 2. (Half-way centre to
edge) . . . = + 27·8 lignes.

No. 3. (Three-quarters centre
to edge) . . . = + 20·8 „

No. 4. (Close to edge). . . = - 5·5 „ (?) }
No. 5. (Close to corner) . . . = - 8·6 „ (?) } reversed.

With *all* the holes open = + 17 lignes.

Of these figures No. 4 and No. 5 appear open to question; No. 5 *should be* less—measured negatively—than No. 4. This progression is not confirmed by his other experiments, and it would appear that there must be a clerical error somewhere. Comparison with his next experiment would lead one to think that the figures are probably reversed—4 for 5 and 5 for 4. The *true* average pressure over the whole plate may, or may not, be = + 17 lignes. Too much importance should not be attached to this exact figure: it is

perhaps curious that it should be so near Newton's estimate of what it should be when a circular plate moved through water at "the speed corresponding to the depth" ($v = \sqrt{2gh}$), viz., about half the hydrostatic pressure. Dubuat's experiment certainly may be taken as showing that the total pressure on the plate was *considerably less than the hydrostatic*.

The next point Dubuat examined was, if the pressure on the anterior surface was affected by the shape of the posterior part of the body. There appears no *primâ facie* reason why it should be, except that the velocity of the stream at the edges would probably be slightly reduced when the body is lengthened. Dubuat fixed a cube at the back of his thin box and found no essential difference. Referring to the previous diagram, the pressures recorded were—

No. 1. + 32·8 lignes.	No. 3. = + 17·8 lignes.
No. 2. + 30·2 „	No. 4. = - 12·1 „

All the holes open = + 14·3 lignes.

Again, with a parallelepiped of proportions $3 \times 1 \times 1$:

All the holes open = + 16·0 lignes.

The conclusion he arrived at was: "En tenant compte des erreurs inévitables dans ces sortes d'expériences, on peut affirmer que la longueur des corps n'influe point sur la pression antérieure."

It may be argued that in all these cases the bodies are at rest and the liquid is moving. Would the same thing occur if the plate were moving and the water were at rest? It is difficult to imagine why it should not be so, and Professor Osborne Reynolds has shown a very beautiful little experiment which will prove that the phenomena depend only on the relative motion of body and fluid.

To a small wooden float attach a little circular disc by means of fine wires (as in the sketch, fig. 16). If this be put in water and the float be given a *sharp* push—*releasing it instantly*—the combination will be found to travel across the tank *as if it were meeting with no resistance*. This may

be said to prove nothing; but if the float be *stopped* and *released at once*, the combination will start again, and go on merrily. Why does it start itself? Obviously because the pressure on the rear face is in excess of that on the front face. As the pressure on the rear face cannot, conceivably, be greater than the hydrostatic pressure, it is clear that the pressure on the front face *must be less than this*, more especially as the float undoubtedly causes *some* resistance.

There is evidently something curious happening at the rear side of the disc, but what that is will be considered later.

Notwithstanding all the evidence to the contrary, it is

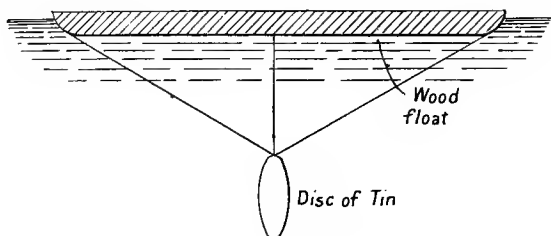


FIG. 16.

commonly taught that the pressure is *augmented* on the front face when a body is moving in a liquid. For example: "The resistance experienced by bodies of imperfect [*sic*] form is due to the *work done on the fluid*, which is not subsequently given back, as in the case of the stream-line body. This resistance can be traced to two causes, namely, *excess pressure on the surface in presentation* and *diminished pressure in the dead-water region*" (Lanchester, *Aerodynamics*, 1907).

How can the small lamina and float experiment be explained by this? I suppose the lamina is an "imperfect" shape, since it is very far removed from the ichthyoid. The pressure is not greater in front than behind, but the reverse.

Also: "Now, as the body advances, the head being

subject to pressure *in excess of that due to the hydrostatic head, etc.*" (same author and book). Examples might be multiplied, but these should suffice, as the work referred to is of late date and by a recognised author.

Since the motion of water on the anterior side of a body is always "steady motion," and since "it seems probable, almost certain indeed, . . . that the steady motion is stable for any viscosity, however small" (Kelvin), it may be taken that it will behave in an exactly similar manner to a perfect fluid, *on the anterior side* of a body.

The deductions that we may fairly draw from the foregoing are—

1. That the shape of the head of a body is not of *very great* importance in increasing or reducing resistance—*provided always that there are no sharp angles and that the head is symmetrical.*

Colonel Beaufoy, long ago, remarked the same thing, for he said: "A convincing proof of the advantage curved lines have over rectilineal ones in dividing the fluid; and this circumstance strongly corroborates the assertion made by many professional men that a *full bow* has the preference to a *lean one* in point of sailing; but it is to be understood that the bow should be made *nearly circular to gain this superiority.*"

2. That whenever an immersed body and the water are in relative motion, what may be called a "liquid prow" is formed: to interfere with this formation cannot be right. A sharp bow, or point, on the front of a body *must* frequently act as a "sharp edge," and so, if not cause, at least increase the "negative righting moment" previously referred to.

It has been found, practically, that a torpedo with a blunt nose travels faster than one with a sharp point, and it also steers better.

What may we expect would be the best head for a torpedo, say? Obviously the one which generates the minimum amount of *kinetic* energy. This should be calculable; and the shape of the head would appear to be some form of

spheroid.¹ Mr Bairstow (Bluebook, *Aeronautics*, 1910-11) says that "a comparatively short head of about one and a half diameter was *satisfactory*."

M. Albert Bazin has lately suggested a method (which may or may not be original) of finding the shape of least resistance *practically*. He says: "Prenez un pain de savon parallépipédique: fixez-le au bout d'une longue ficelle A B

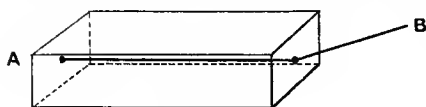


FIG. 17.

comme ci-contre (fig. 17): puis remorquez-le (autant que possible hors du sillage) dans l'eau, derrière un bateau rapide; au bout de quelques minutes retirez-le; il aura pris *naturellement* une forme de fuseau analogue à celles que vous avez dessinées et il le *conservera tant qu'il restera du savon autour de la ficelle*" (Ernoul, *L'Aviation de Demain*, 1910).

SUMMARY

Water flowing to meet a body, even when the liquid has a free surface, behaves like the mathematicians' perfect fluid—subject to some *slight* variations caused by viscosity. The difference might, in this case, be popularly described as the

¹ Mr Lewis R. Shorter has very kindly calculated for me the equatorial velocity of a stream flowing past an ellipsoid of revolution, the direction of flow at infinite distance being parallel to the axis of revolution and its magnitude V , from the very formidable-looking formula of Lord Kelvin.

$$EV = \frac{(a^2 - b^2)V}{b \left\{ \frac{a^2}{\sqrt{a^2 - b^2}} \cdot \frac{\sin^{-1} \sqrt{a^2 - b^2}}{a} - b \right\}}$$

	Ratio of Axes.	Equatorial Velocity.
1. <i>Prolate spheroids</i>	$\infty : 1$	1'00 V.
	3 : 1	1'12 V.
	2 : 1	1'21 V.
2. <i>Sphere</i>	1 : 1	1'50 V.
3. <i>Oblate spheroids</i>	1 : 2	2'12 V.
	1 : 3	2'74 V.
	1 : 100	64'47 V.
	1 : ∞	∞ .

difference between the reflection from a flat mirror and from one which is *very slightly curved* cylindrically.

The pressure on the anterior side of a body is *less* than the hydrostatic pressure, whether the body moves past the liquid, or the reverse.

The pressure on the centre of a circular plate broadside on to a flowing stream is the hydrostatic pressure: this pressure decreases from the centre to the edge of the plate.

Any sharp edge in the head of an immersed body appears to produce increased resistance and to cause, or increase, a "negative righting moment": *i.e.* tends to make the motion unstable.

REFERENCES

As before.

CHAPTER VIII

RESISTANCE DUE TO VISCOSITY VARIES, NOT AS THE
"WETTED SURFACE," BUT AS THE "SHEARING SUR-
FACE" AND AS THE VELOCITY ONLY

IT will be well now to consider how far this theory agrees with experiment.

In 1872 Dr Froude published the results of some very fine experiments he had conducted on the resistance of long flat boards towed edgewise. These are still the classical experiments which are always referred to when mention is made of liquid friction. He gives the curves of resistance at varying speeds.

Selecting curve B on Plate IV. (I select this as a specially good curve, since all the observed spots are accurately on the curve), the following resistances can be measured off at the speeds of 100, 200, 300, 400, 500, 600, 700, and 800.

(Board, 28 ft. long, varnished surface.)

At 100 ft. p.m. resistance = 9 lbs.

" 200	"	"	= 3'4 "
" 300	"	"	= 7'1 "
" 400	"	"	= 11'7 "
" 500	"	"	= 17'5 "
" 600	"	"	= 24'6 "
" 700	"	"	= 32'7 "
" 800	"	"	= 42'4 "

If we examine these in the ordinary way—assuming that

they vary as *some* power of the velocity—it will be seen that they vary—

Between 100 and 200 as V^2 (nearly).

„	200	„	300	„	$V^{1.82}$
„	300	„	400	„	$V^{1.73}$
„	400	„	500	„	$V^{1.80}$
„	500	„	600	„	$V^{1.86}$
„	600	„	700	„	$V^{1.84}$
„	700	„	800	„	$V^{1.94}$

No special law is apparent, though it is customary to say the variation is as $V^{1.83}$ (some American books put it more neatly as V^2), the differences being put down to “error.” Not only is this unscientific, but it completely masks the dimension law.

If we examine them by the formula $AV + BV^2 = R$, putting V as 200, 400, 600, and 800, it will be found that A and B are constants, as they should be, and that they have approximate values—

$$A = .005, \quad B = .00006.$$

Applying these constants to the formula for the velocities 300, 500, and 700, we get

At 300 $R = 7.1$ calculated as against 6.9 from curve.

„ 500 $R = 17.5$ „ „ 17.0 „

„ 700 $R = 32.7$ „ „ 32.9 „

An exceedingly close agreement for such very small curves, which were *probably drawn by means of a bent lath*.

To show that this is not an accidental coincidence, let us take the next curve, CC, also Plate IV. (16 ft. long only, but also with varnished surface)—

At 200 ft. $R = 1.95$ lbs.

„ 300 „ $R = 4.15$ „

„ 400 „ $R = 7.0$ „

„ 500 „ $R = 10.6$ „

„ 600 „ $R = 15.0$ „

„ 700 „ $R = 20.0$ „

„ 800 „ $R = 25.4$ „

Putting the different values of V and R in the equation $R = AV + BV^2$, as before, it will be found that

$$A = \cdot 00325, \quad B = \cdot 000037,$$

will satisfy the equation within limits of error.

To compare the constants in these two curves it must be remembered that BB is the curve of resistance of a board 28 ft. long, whilst CC is that of a board of 16 ft. only. Reduced to the same length, it will be found that A is practically the same for both experiments, *whilst B is not.*

In the curve AA, Plate IV. (board 50 ft. long, varnished surface), we find

At 100 ft.	R = 1·4 lbs.
„ 200 „	R = 5·3 „
„ 300 „	R = 11·2 „
„ 400 „	R = 19·0 „
„ 500 „	R = 28·3 „
„ 600 „	R = 39·8 „

Where $A = \cdot 0089$ and $B = \cdot 000095$ will satisfy the curve very nicely. Here, again, A has increased in the ratio of 50 : 28 from the value in the curve BB, or *as the length*: B is less than it should be if this ratio were maintained.

Curve DD, Plate IV. (5 ft. long, varnished surface), examined in the same manner—

$$A = \cdot 0009, \quad B = \cdot 0000155.$$

	From curve.	Calculated.
At 400 ft.	R = 2·7 lbs.	2·87 lbs.
„ 500 „	R = 4·3 „	4·32 „
„ 600 „	R = 6·2 „	6·12 „
„ 700 „	R = 8·4 „	8·22 „
„ 800 „	R = 10·7 „	10·67 „

Here, again, A is reduced as 5 : 28 when compared with the BB curve.

Plate V., curve AA (2 ft. 6 ins. long, varnished surface). As before—

	From curve.	Calculated.
At 300 ft. R=	·9	·924
„ 400 „ R=	1·56	1·376
„ 500 „ R=	2·4	2·375
„ 600 „ R=	3·4	3·366
„ 700 „ R=	4·56	4·529
„ 800 „ R=	5·88	5·864

When $A = \cdot 00045$ and $B = \cdot 000086$.

Here, again, A is reduced proportionally to the reduction in the length of the board.

Plate V., curve B B (1 ft. 6 ins. long, varnished surface)—

$$A = \cdot 00027 \quad \text{and} \quad B = \cdot 0000516.$$

Plate V., curve C C (1 ft. long, varnished surface)—

$$A = \cdot 00018 \quad \text{and} \quad B = \cdot 0000035.$$

An examination of all these results will show that in *all cases* A varies as the length of the plane. This is commonly stated as varying as the *wetted surface*. This is not correct, for it should be as the *shearing surface*. It is true that in this case the wetted and shearing surfaces are the same; but there are cases where they are very different, as will appear later. We may, therefore, replace A in the equations by aL , where a is a small constant for *all the curves*, and L =length; and we may rewrite the equation

$$R = aLV + BV^2,$$

where, however, the value of B is different for different curves.

In this equation $a = \cdot 00018$ or 18×10^{-5} *per foot of length*. Since the girth of the boards was 3 ft. 2 ins., the value of a *per square foot* would be $5\cdot7 \times 10^{-5}$.

To understand the subject properly it will be necessary to enter into a little history.

Coulomb first made experiments with discs caused to oscillate in oil.

“In Coulomb’s experiments it appeared that the resistance was composed of two terms, one involving the first power, and the other the square of the velocity” (Stokes, *Math. Works*, vol. iii.).

Coulomb believed that at *very low velocities* the part of the resistance *depending upon the square of the velocity* was so small that it *might be neglected*. Stokes appears to have held the same view, for he says that "in the case of minute globules falling with their terminal velocity, *the part of the resistance depending upon the square of the velocity*, as determined by

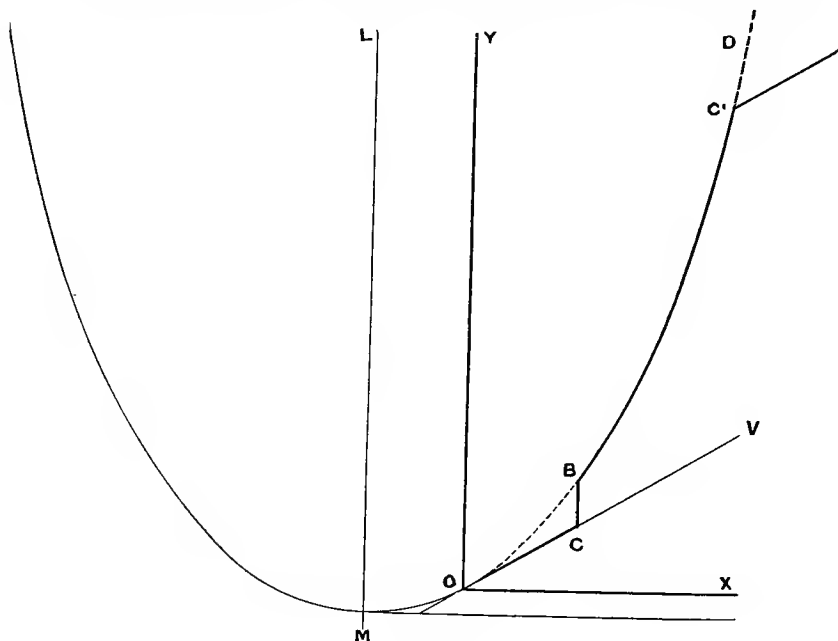


FIG. 18.

the common theory, is quite insignificant compared with the part which depends on the internal friction of the air."

All this is ancient history, for Stokes wrote in 1850. We know now, from Professor Perry's experiments, that at *very low velocities* (below a critical speed) the part of the resistance depending upon the square of the velocity *does not exist*. $R = AV$ *accurately*, but above this critical speed the resistance *suddenly increases*. Referring to this suddenness, Osborne Reynolds says: "It was a matter of surprise to me to see the sudden force with which the eddies sprang into

existence, *showing a highly unstable condition to have existed at the time the steady motion broke down.*"

It is clear, therefore, that as the resistance varies *as the velocity* only, to begin with, and as the BV^2 factor is only, what I may call, "switched in" at the *critical velocity*, the inertia resistance does not commence at the origin but at a point on the $R=AV$ line which corresponds to the critical velocity. The curve of velocity-resistance, which is represented by $R=AV+BV^2$, is, of course, a parabola, which passes through the origin and which is tangential to the line $R=AV$ at the origin. This may be represented graphically, as in the diagram (fig. 18), where $MOBD$ is the parabola, tangential at O to the line OV . C represents the point on the line $R=AV$ which corresponds to the critical velocity. The resistance will therefore be as follows:— A to C (critical velocity), $R=AV$. At C the resistance increases, *suddenly*, to B , and it then follows the parabolic curve along BD . From A to C the liquid is stressed and is in the *highly unstable condition* referred to by Osborne Reynolds.

The diagram is not to scale and is only intended to illustrate my meaning.

The law of the variation of the parameter B in the BV^2 term is decidedly puzzling.

For 1 ft. 0 ins. $B=3.5 \times 10^{-6}$ per foot of board.

1	„	6	„	$B=3.44 \times 10^{-6}$	„	„
2	„	6	„	$B=3.44 \times 10^{-6}$	„	„
5	„	0	„	$B=3.1 \times 10^{-6}$	„	„
16	„	0	„	$B=2.31 \times 10^{-6}$	„	„
28	„	0	„	$B=2.14 \times 10^{-6}$	„	„
50	„	0	„	$B=1.9 \times 10^{-6}$	„	„

In other words, up to 2 ft. 6 ins. B increases *as the length of the board* almost exactly. After the length 2 ft. 6 ins. is reached B no longer increases proportionately to the length, but at a slower rate; and the rate decreases as the length is increased.

The only explanation I can offer is that the planes vibrate like the "Bull roarer," and that, as they vibrate more slowly

with increased length, the inertia resistance, caused by the formation of eddies or vortices, also increases more slowly.

Until a better explanation is offered, this will serve as a good working hypothesis. That the plate *does* vibrate may be taken as a fact. It was observed by Beaufoy, who says: "Almost all the figures, when drawn through the water with a certain velocity, acquired a violent tremulous motion, which, in a small degree, may be illustrated by observing a large pot on the fire when it simmers." Mr Mallock (*Aeronautics*, Bluebook, 1909-10) also says: "A flat surface when towed by the leading edge is exceedingly unstable."

That the boards *must* vibrate can easily be shown to be a necessary consequence of theory. If we imagine A B in

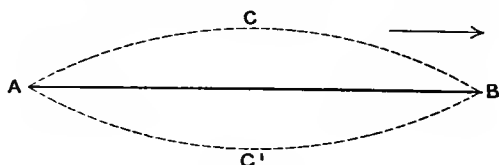


FIG. 19.

the diagram (fig. 19) to represent the board, which is held fast at B and being towed in the direction of the arrow. We next imagine that, *for some reason or another* the stream-lines flow faster on one side than the other—say past C. There will be less pressure on the C side of the board than on the other, and the board will bend towards A C B. This will further increase the velocity of flow past C, and so further reduce the pressure: the board will bend more towards C. Eventually the water on the *other* side will be "cracked" at B; discontinuity will be produced and the board will fly over to A C' B, and so on. A vibratory movement of the board is thus set up, its period being a function of the dimensions of the board.

From the foregoing it may be seen that the *resistance due to viscosity* (which in the last analysis is a stress set up to *resist shearing* of the liquid) of a plate drawn edgewise through water varies *as the velocity* and *as the shearing surface*. In this case the wetted surface and shearing surfaces are the same.

The law of this resistance, as commonly taught, is that it

varies as $V^{1.83}$; it may, of course, vary as $V^{1.83}$ at certain speeds, but certainly not for all speeds, or beyond very narrow limits. I maintain that this $V^{1.83}$ law (??) has a further defect, that it completely masks the simple "law of dimension" which is here pointed out: hence it is commonly stated that the resistance due to viscosity does *not* vary as the length, whereas I here show it does.

There is another law of liquid resistance which has latterly been introduced by Mr Lanchester in his *Aeronautics*. He says the curve of resistance commences as a straight line, (1) thus following "Stokes' law"; after a critical speed, which he does not state, it changes to $R \propto V^{1.5}$, (2) which he calls "Allen's law"; it later changes again to $R \propto V^2$, (3) which he calls "Newton's law."

(1) I am unable to find that Stokes ever said that the resistance *varied as the velocity*. He said that at very low speeds the resistance *depending upon the square of the velocity* was so small that it *might be neglected*; but that is not the same thing at all. It is curious to find Mr Lanchester speaking approvingly of Stokes' law, for he certainly gives it a very severe condemnation when he says: "Stokes' law is based on a system of motion of the fluid that has been mathematically investigated and the lines of flow plotted from an equation. It may be remarked that this system of motion *can never exist in its entirety*, for it involves an infinite quantity of momentum and an infinite quantity of energy: in other words, the steady state involves a force applied for an infinite time through an infinite distance; it also constitutes a violation of the *principle of no momentum*." So much for Buckingham!

(2) Allen commences by saying that "*if* the resistance *can* be represented by a single term it must be proportional to $(aV)^n$." He then proceeds to show that it varies approximately as $V^{1.5}$. His experiments¹ are not convincing, for they are subject to the following errors, which he admits:—

1. The reduction of pressure, as the bubbles rise, causes the bubbles to *enlarge*.

¹ On the time taken by small bubbles of air in rising through a cylindrical vessel of water.

2. The gradual solution of the gas in the liquid causes the bubbles to *diminish* in size.

3. The bubbles are skimmed off the surface of the liquid and measured. The range of velocities is exceedingly small: from .12 inches per second (leaving very little room for Stokes' law) up to a maximum of 2.2 inches per second. The law (?) appears to have no very great *practical* importance.

(3) Newton's law does not appear to have been that the resistance varied as the *square of the velocity*, except in fluids which have no "lubricity." The actual curve of resistance does not follow this law *except in special cases*. The general approach to it is about $V^{1.83}$. Newton undoubtedly says, in places, what might be supposed to mean that $R \propto V^2$; but nothing can be clearer than his scholium: "The resistance of spherical bodies in fluids arises partly from the tenacity, partly from the attrition, and partly from the density of the medium. And that part of the resistance which arises from the density of the fluid is, as I said, in a duplicate ratio of the velocity, *the other part* [*italics added*], which arises from the *tenacity of the fluid*, is uniform, or as the moment of the time."

Why, then, should it be taught that the resistance varies as $V^{1.83}$? It is very difficult to say. There appears to be a fascination about the idea that the resistance varies as *some single power* of the velocity. Undoubtedly, *within narrow limits*, it is always possible to find *some* logarithmic curve which is *nearly* the same as $R = AV + BV^2$; but the whole process reminds me of the way carpenters strike an ellipse (?) with a compass; by piecing together small pieces of circles with different radii, they describe a curve which—to the *un-trained eye*—looks very like an ellipse!

Mr Lanchester's ideas appear clear enough: "In all cases of *purely viscous resistance* the law of viscosity requires that the resistance shall vary *directly as the velocity*; and the *whole of the energy* expended *disappears at once into the thermodynamic system*. In cases where the resistance is *dynamic*—that is to say, where it is due to the *continuous setting of new masses of fluid in motion*—the *whole of the energy* expended

remains in the fluid in the kinetic form (being only subsequently frittered away), and the resistance varies as the square of the velocity. When the resistance is due to both causes combined, as in the case of skin friction, the portions of the total resistance varying directly, and as the square, are respectively proportional to the energy expended in the two directions" (*Aerodynamics*, italics added).

Nothing could apparently be clearer; but in case anyone should think the end of the last paragraph the least bit vague, he says later:

"Let R_1 be the resistance varying as V_1 , and R_2 be the resistance varying as V^2 , then $R = R_1 + R_2$."

Notwithstanding this exceedingly clear presentation of the case, Mr Lanchester, by some curious train of reasoning, comes to the conclusion that $R = V^n$, the argument commencing: "Now for any particular velocity the total resistance—that is, the sum of the viscous and dynamic resistances—may be expressed as varying as the n^{th} power of the velocity." This, of course, is a truism; for $V^n = \text{anything you like}$. If we take, say, $V = 100$; then, if $n = 2$, $V^n = 10,000$; if $n = 1$, $V^n = 100$; and if $n = .5$, then $V^n = 10$.

What appears fairly certain is that if $R = R_1 + R_2$, then R varies as no single power of the velocity, and Allen's law is not true.

In the next chapter Stokes' law will be specially referred to, as it is very pertinent to the law of resistance which has been discussed here.

SUMMARY

The resistance of a long flat board drawn edgewise through water may be expressed by $R = AV + BV^2$, where A and B are constants for each flat plane, being what are called "parameters."

For A we may substitute aL , where a is a constant for all the planes and L is the length of the plane, the resistance due to viscosity (AV) varying as the velocity and as the shearing area.

The coefficient B of the inertia resistance (or *dynamic* resistance), term BV^2 , varies in a curious manner which is not easy to explain. The variation appears to depend on the length of the board, and—(?) probably—on the *period* of the vibration of the board transversely to the line of motion, this vibration being a necessary consequence of the theory of hydrodynamics.

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CHAPTER IX

RESISTANCE DUE TO VISCOSITY (*continued*)— STOKES' LAW

IN the last chapter it was shown that when thin plates are drawn through water that the resistance *due to viscosity* varied as the velocity and as the shearing surface. All the curves of resistance of boards *of the same kind*, in Dr Froude's paper, were examined and gave the same results. These planes were all varnished; but others, covered with Hay's composition, give almost exactly the same results. There are, however, some curves, in the same paper, of the resistance of planes covered with tin-foil where *A* is *very much* less than in the curves examined. For instance, the plane corresponding to curve *C'' C''*, Plate IV., is of the same length as that referred to in curve *CC*; and the same holds for curves *B' B'* and *C' C'*, Plate V., as compared with the curves *BB* and *CC* in the same plate. In all these cases it will be found that *A* is, roughly, from 45 to 50 per cent. less than it is in the "varnished plane" curves. The reason for this appears to be that the tin-foil is very imperfectly "wetted" by the water. Put in other words, the *adhesion* of the water to the tin is less than it is to a varnished surface. When this adhesion is imperfect, the liquid slides on or rolls over the solid, and the resistance is no longer the same. It is probably rolling and sliding a *little* as it rolls. I should liken it to the action of a bicycle tyre on a sticky road. The tyre is always in contact with the road, but whilst it rolls along it sometimes slips as well when going down a slope.

We see, therefore, that Professor Perry's rule 4: "The force

of friction at moderate speeds does not much depend on the nature of the wetted surfaces," is not strictly accurate. It requires to be qualified by—*provided the surfaces are equally or perfectly wetted.*

His rule 3 also requires a trifling modification: "The force of friction is proportional to the *shearing* [not wetted] surface"; though in the cases he refers to the two are the same.

It may be well to try and get some clear mental image of how this treaciness or viscosity acts in causing a resistance to a plate. Amongst naval architects, who speak of "skin friction," it is commonly supposed to be caused by the water "rubbing" against the side of the ship, and tables have

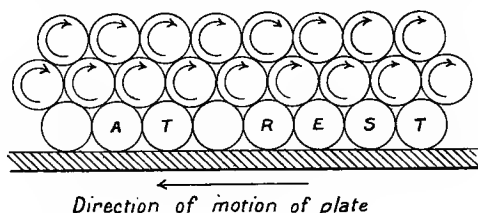


FIG. 20.

been made of the friction of water against different substances. Even Dr Fleming, when referring to the resistances of a boat (*Waves and Ripples*) says: "These are, first, the skin of friction *between the ship's surface* and the water"—he was probably speaking in a very "popular manner."

Professor Lamb (*Hydrodynamics*) goes a little to the other extreme, for he says: "It is probable that in all ordinary cases there is no motion, relative to the solid, of the fluid immediately in contact with it. The contrary supposition would imply infinitely greater resistance to the sliding of one portion of the liquid past another than in the sliding of the fluid over the solid." The truth would appear to be probably somewhere between these two views.

Let us imagine the sketch (fig. 20) to represent (very diagrammatically) some particles of water and a moving plate. We may assume the layer next to the plate to be

absolutely at rest, relatively to it, and consequently moving at the same velocity. The next layer will move *slightly slower* than the plate, rolling along the particles at rest. The next layer will roll on this layer, and so on. It will be evident that these particles in rolling will rub against one another and so generate heat which will be dissipated.

We may put the matter in a different way. Let us imagine, in this case (fig. 21), the water to be moving and the wall to be at rest. Let A B C D represent diagrammatically a longi-

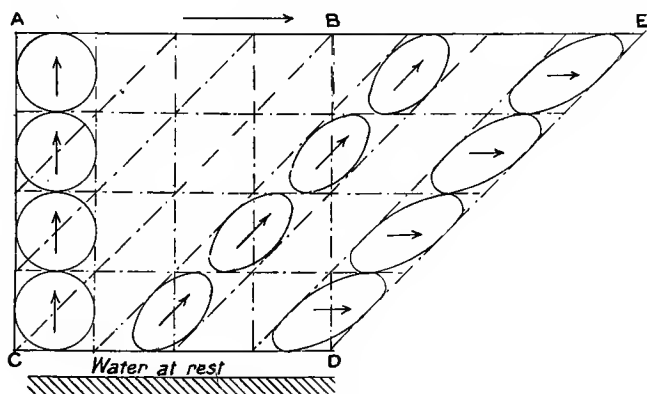


FIG. 21.

tudinal section of a body of water moving in the direction of the arrow; C D representing the surface of the molecules of water which are "at rest."

After a small period of time this body of water will have changed its position to B C D E, when all the particles of water in contact with those at rest will have rolled a finite distance along C D, and *all* the particles will have *rotated through the same angle*, as shown by the small arrows.¹

Let us now imagine the water, which is *now* at A B C D,² to be moved in the same manner. The "first layer" particles will roll still more along C D in the direction of arrow, and

¹ The diagram shows the particles as deformed: such is not intended, for the angle A C B is supposed to be *exceedingly small*.

² Not necessarily the *same particles*.

all the particles will rotate still more through the same angle, as shown by the small horizontal arrows.

Repeat this process indefinitely, and the reader can imagine all the particles moving along in the direction A B, and *all rotating at one uniform velocity.*

If the particles near A B move *too fast* the "first layer" particles will slip, as well as roll over, the particles at rest. This is what has actually been found to be the case in very large rivers.

If we next suppose the adhesion of the liquid to be imperfect, we can imagine the layer marked "at rest," as also rolling slightly along the plate—not necessarily at the same rate as the other layers. If the adhesion is very small we can further imagine the particles next to the plate as, not only rolling, but actually *slipping* along the plate.

A step further: if the adhesion be quite negligible—as between clean mercury and glass—then the liquid will slide along the plate and there will not *necessarily* be any rolling at all.

This appears a fairly satisfactory working hypothesis of the molecular action of a liquid against a plate. This explanation is consistent with the well-known fact that a *slight* roughness of the surface will not increase the resistance. Poiseuille placed a small quantity of very finely powdered shellac in a glass tube, and took the time required to discharge a certain amount of water through the tube with a fixed pressure. He then heated the tube so as to melt the shellac and make it quite smooth: the time of discharge was unaltered.

If the liquid do *not* wet the body, as, for example, in the case of mercury flowing through a glass tube, then a layer of air would be interposed between the mercury and the glass, and the friction would be an "air-air" one, depending upon the viscosity of the air. This Girard found so small, in fine tubes, that he could not measure it, and he gives the resistance varying *strictly as BV^2* only.

If the foregoing be correct, there are certain practical deductions we can draw.

If frictional resistance depends on the body immersed

in the liquid being *wetted*, then, if we could prevent a ship from being wetted—or even reduce the perfection of the wetting—it ought to meet with less resistance. This is, of course, done, to some effect, by copper sheathing (though this was adopted for other reasons); but if we could coat the ship with a layer of air, the resistance, being “air-air,” should be very much less than “water-water.” This was actually tried some years ago, a layer of compressed air being interposed between the bottom of the vessel and the water. Sir Frederick Bramwell, who described the experiment, said the effect was peculiar and “soda-watery,” but the resistance was *materially reduced*. Scientifically the experiment was a success, though it was a failure commercially; for the power expended in compressing the air was so great that there was no advantage measured in *£ s. d.*

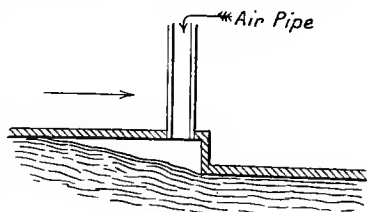
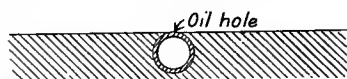


FIG. 22.

Very much the same result is achieved in the racing boats called “hydroplanes.” If the diagram (fig. 22) represent a small piece of the bottom of the boat, travelling to the right, as represented by the arrow, the water moving quickly past the “step” of the hydroplane will cause air to flow through the air-pipe into the step. The boat may be said to be partly “floating on air.” These boats have achieved a great measure of success. Sir John Thornycroft’s last pattern (the maple leaf) is said to have attained a speed of 48·8 knots.

There is another way in which reduction of resistance might be tried: we might imitate Nature. A fish is covered with oil glands which keep its body well greased. It would not be very difficult to arrange small oil tubes round a ship, with very fine holes in them (fig. 23). This might be worth trying (on yachts or torpedo boats), as the consumption of oil need only be comparatively trifling. The old whaling captains used to declare that whalers were always faster than sister ships; and they attributed this to the sides being always oily.

It must not be imagined that if the wetted surface of a body be increased that *therefore* the resistance will be increased. Very often the reverse is true, and Beaufoy



Side of Ship

FIG. 23.

says: "Among the conclusions suggested by the tables [in his book], one of the most curious is, that the increasing the length of a solid of almost

any form by the addition of a cylinder in the middle exceedingly diminishes the resistance with which it moves, provided the weight in water continues to be the same—a fact, I apprehend, that cannot be easily explained." One of the examples he gives is—

Resistance of a sphere = 62·85 lbs.

Sphere cut in half and cylinder inserted ($l=d$), where l =length of cylinder and d =diameter of sphere—

Resistance = 49·71 lbs.

It is well known, also, that many ships have been lengthened by the insertion of a middle piece, with the result that they attained a greater speed.

A very curious example of the difference between the action of a body in a liquid, when the body was wetted and when it was not, was first shown by Professor Worthington at the Royal Institution in 1894 (*Splash of a Drop*, S.P.C.K.), and is as follows:—Take a small *very smooth* marble of

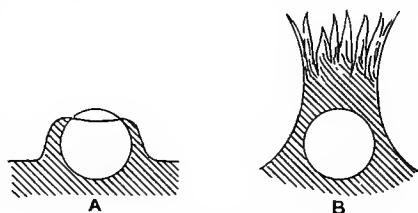


FIG. 24.

about $\frac{1}{2}$ inch diameter: dry it thoroughly and slightly warm it. If this marble be dropped from a height of 2 or 3 feet into a bucket of water, the water will spread "over the sphere so rapidly that it is sheathed with the liquid even before it has passed below the general level of the surface" (fig. 24, A). "The splash is insignificantly small and of very short duration."

If the marble be wet, however—the same marble picked out of the bucket—it will give a very considerable splash (fig. 24, B).

I think I have observed the same sort of effect on armour plates struck by steel and bronze shot. In the latter case the “splash” of the plate was very much less than when it was struck by the steel projectile. The bronze shot was probably not so much “wetted” (if one may use the word) by the steel of the armour plate as in the case of the steel projectile. In other words, the adhesion of steel to bronze was less than steel to steel.

STOKES' LAW

It is very curious how many people refer to Stokes' law and how few really know what it is. It appears to be generally believed that Stokes said that at very low velocities the resistance to bodies varied *as the velocity only*. I am not aware that Stokes ever said anything of the sort: I have not seen it after careful study of his papers. What he did say was (*Math. Papers*, vol. iii.): “The resistance of a sphere moving uniformly in a fluid may be obtained as a limiting case of the resistance of a ball pendulum, *provided the circumstances be such that the square of the velocity may be neglected* [italics added]. The resistance thus determined proves to be proportional, for a given fluid and a given velocity, *not to the square*, but to the *radius of the sphere*; and therefore the accelerating force of the resistance increases much more rapidly, as the radius of the sphere decreases, than if the resistance varied *as the surface*, as would follow from the common theory. Accordingly the resistance to a minute globule of water falling through the air with its terminal velocity *depends almost wholly on the internal friction of air*. Since the index of friction of air is known from pendulum experiments, we may easily calculate the terminal velocity of the globule of given size, *neglecting the part of the resistance which depends upon the square of the velocity*. . . . Since in the case of minute globules falling with their terminal velocity *the part of the resistance depending upon*

the square of the velocity, as determined by the common theory [$R = AV + BV^2$], is quite insignificant compared with the part which depends on the internal friction of the air, it follows that were the pressure equal in all directions in air in the state of motion, the quantity of water which would remain suspended in the state of cloud would be enormously diminished."

The paper is exceedingly mathematical and difficult to follow, but the "law" is quite clear from the foregoing quotation. Put in other words, when the velocity is *very low* the resistance of a sphere varies (*practically*) as the velocity and as the radius of the sphere. The reader will see at once that this law is a special case of the general law here enunciated that the resistance varies as the velocity and as the shearing surface. In the case of a sphere the shearing surface is obviously a band round the equator, which, of course, varies as the radius. If the resistance due to viscosity varied as the wetted surface, then it would vary as r^2 .

Stokes' law may have all the vices attributed to it by Mr Lanchester, and quoted in the last chapter; but it at least has the merit of agreeing with experiment, which some laws occasionally do not. It appears capable of a rational explanation; and it further indicates *why* Newton's experiments with spheres tended to show that the resistance varied as the square of the velocity. It is clear that since the "shearing surface" of an oblate spheroid moving along its axes of revolution is exceedingly small—smaller than that of any other body (a sphere may be considered as an oblate spheroid of revolution where the axes are as 1 : 1)—then in the formula $R = AV + BV^2$, AV , the resistance due to the viscosity, will be very small; and the resistance of the sphere will approximate to BV^2 . If anyone wants to prove that the resistance of bodies varies as the square of the velocity, he should select a sphere or a thin lamina to experiment with.

The whole subject of viscosity wants overhauling very badly. If we accept Newton's hypothesis that "the resistance arising from the want of lubricity in the parts of a fluid is, *cæteris paribus*, proportional to the velocity with which the parts of the fluid are separated from each other," then it will

be evident that the method of measuring the coefficient of viscosity by allowing liquids to flow through capillary tubes and observing the time of flow is *not a sound one*. It is clear that the flow of water through a glass tube will not be the same as that through a tin tube *of the same dimensions*. Some liquids adhere to glass much more strongly than water does, and so would be credited with a higher coefficient of viscosity than they actually possess. For example, Girard found that a strong syrup of sugar and water flowed more freely through a small glass tube than alcohol. If the liquid *slips* along the glass, then we are not measuring the *cohesion of the liquid*, but something very different.

It will probably be objected that Professor Lamb, having discussed the "steady flow of a liquid through a straight pipe," says in reference to his formulæ:

"This last result is of great importance as furnishing a *conclusive proof* that there is in these experiments [of Poiseuille] no *appreciable slipping of the fluid in contact with the wall*" [italics added]. Also: "The assumption of no slipping being *thus justified*, the comparison of the formula with experiment gives a *very direct means of determining* the value of μ for various fluids."

Professor Lamb is a very eminent mathematician and the distinguished author of the great classic on *Hydrodynamics*; it requires therefore considerable audacity to question anything he says on his special subject. Still, "there is no more common error than to assume that, because prolonged and accurate mathematical calculations have been made, the application of the result to some fact of nature is absolutely certain. The conclusion of no argument can be more certain than the assumption from which it starts" (Whitehead, *Introduction to Mathematics*).

If we grant Professor Lamb's premises, there is no getting away from his conclusions. His view appears to be as follows:—If we imagine the diagram (fig. 25) to represent the longitudinal section of a piece of the tube, and A, B, C, D, etc., to represent sections of the very thin co-axial tubes of water. The tube A, not slipping, will have a velocity = 0, referred to

the wall. Expressing the velocity of B as 1; then, if this be supposed very small, the velocities of C, D, etc., may be represented by 2, 3, etc.

If we next imagine A to slip, so as to have a velocity = 1, then the velocities of B, C, D, etc., will be represented as 2, 3, 4, etc. This flow would be similar to that which would occur in a larger tube where there was another liquid cylinder, outside A, which did not slip.

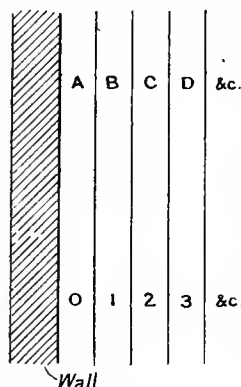


FIG. 25.

Now, what are the assumptions implied in this argument? Clearly, that there is attraction between the different layers of liquid, and that there is attraction between the layer A and the wall.¹ But if there is attraction between A and the wall, there must also be attraction between B and the wall (though in a less degree), and similarly between C, D, etc., and the wall. This does not appear

to have been taken into account. If we bring this factor in, we can easily imagine that A is slipping (to some appreciable extent), whilst B, C, D, etc., are being slightly *retarded* by the wall, so that the effect of A's slipping might be *completely masked*. The result might be *exactly the same* as that supposed by Professor Lamb, though the reasons for this might be vastly different.

Referring to the second point, it appears only reasonable to suppose that different liquids exert more or less attraction on the wall. If we suppose some liquid to be *greatly* attracted by the wall, we might imagine that not only the velocity of A is *zero*, but also that of B, and possibly of C. In this case we would have a flow similar to that which would occur in a smaller tube, where the *outside layer did not slip*—as if, in fact, the layer A (or the layers A and B) had been “peeled off.”

¹ It may be that even *attraction* of the layer A to the wall is not postulated. The *external friction*—between the liquid and the wall—may be possibly what is intended.

Applying this to the measure of viscosity: suppose we compare two liquids, where, in the first place, $B=1$, and, in the second case, $B=0$. It would be said that the second liquid was *more viscous* than the first, *because it flowed through the tube more slowly*. Of course it flowed more slowly—it *was flowing through a smaller tube!*

The further discussion of this point will be deferred until Poiseuille's law has been explained in Chapter XI.

SUMMARY

The resistance of a body in a liquid depends, to a certain extent, on how far the body is "wetted" by the liquid.

Stokes' law appears to be a special case of the general law that, at low velocities—where the resistance is entirely due to viscosity, and consequently varies *as the velocity* only—the resistance varies *as the shearing surface*. In the case of a sphere the shearing surface is a band round the "equator" of the sphere. The resistance varies as the shearing surface, *not* as the "wetted surface," although it frequently happens that the shearing and wetted surfaces are the same.

The statement that a liquid flowing through a tube does not "slip" along the walls of the tube appears questionable; the subject requires further investigation.¹

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„ *A Study of Splashes*. (Cambridge Press.)

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¹ "According to Helmholtz and Pietrowski, there is slipping between the fluid and the solid, but more between water and silver than between water and glass."—J. G. Butcher, *On Viscous Fluids in Motion*.

CHAPTER X

HOW LIQUIDS CHANGE FROM STEADY TO "SINUOUS" MOTION—VORTICES—SENSITIVE FLAME—SINGING FLAME

IT is well to define what we mean by "sinuous motion" and vortices.

Osborne Reynolds employs the term sinuous motion for all forms of motion where the liquid has broken, or is breaking, into eddies: it will be used here strictly in that sense.

Lord Kelvin says: "I now define a *vortex* as a portion of fluid having any motion that it could not acquire by fluid pressure *transmitted through itself from its boundary*" [italics added]. It will be evident that Lord Kelvin's definition will include all forms of sinuous motion: it is necessary to reflect well over it, for it contains the key to a great difficulty.

Now, how does steady motion change into sinuous motion?

Stokes says (*Math. Works*, vol. i.): "Except in the case of capillary tubes, or, in case the tube be somewhat wider, of excessively slow motions, the *main part* of the resistance depends upon the *formation of eddies*. This much appears clear; but the precise way in which the eddies act is less evident."

Speaking of waves, he says (*Math. Papers*, vol. ii.): "Waves are usually produced either by some sudden disturbing cause, which acts at a particular part of the fluid *in a manner too complicated for calculation*, or by the wind meeting the surface *in a manner which cannot be investigated*."

These statements, made in 1849, are certainly not promising.

In 1899 (Royal Institution) Dr Hele-Shaw remarked:

"Whatever the real nature of viscosity is, it results in *producing in water eddying motion*, which would be perfectly impossible if viscosity were absent"; but it must be remembered that Lord Kelvin has stated that vortices *would* be produced in a non-viscous liquid in certain circumstances, and he has also shown how he conceives they would be formed.

If we refer to Mr Lanchester's *Aerodynamics*, we see that his description of the formation of eddies leaves much to be desired. "If, in the first place, the fluid be taken as inviscid, and if, for the purpose of this argument, we *assume (!) that the system of flow in the figure (fig. 26) is possible in an inviscid fluid*. . . . Let us next introduce viscosity as a factor. The

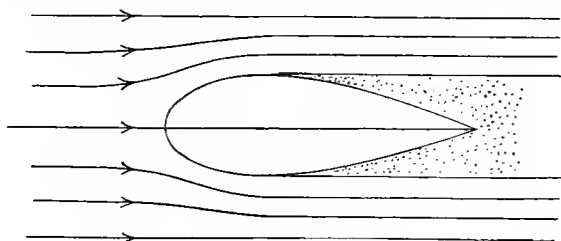


FIG. 26.

conditions are now altered, for the fluid in the dead-water region is *no longer motionless, but is in active circulation*. . . ."

This has almost the appearance of a conjuring trick. The perfect liquid behind the body is *at rest*; introduce a teaspoonful of glycerine, when, presto! the liquid commences to boil. Why?

It is not denied that eddies *can* be produced by viscosity, *under special circumstances*, but it is contended that they are, nearly always, produced in water in a totally different manner; and that there is no reason why they could not, *in similar circumstances*, be produced in a perfect liquid, if such a thing existed. Professor Lamb appears perfectly clear on the subject of the possibility of producing vortices, for he says (*Hydrodynamics*): "It is obvious that *cyclic* irrotational motion of a liquid cannot be reproduced by any arrangement of simple sources. It is easily seen, however, that it may be represented by a certain distribution of double sources."

Remembering Professor Perry's dictum that with liquids it is pressure, *pressure*—always pressure, it is evident that what is required to produce sinuous motion is a *cross pressure* to act as a second "source." A few examples will now be given of the change of direct into sinuous motion.

To take the case of water issuing from, or entering a trumpet mouth. This problem is said to have bothered Dr Froude for a long time; for whilst the liquid could be caused to flow *into* the trumpet at almost any reasonable velocity, and still act according to the recognised laws of the hydrodynamical "drill book," if the motion were reversed

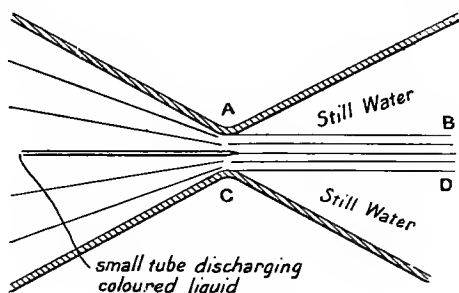


FIG. 27.

it was hardly possible to cause it to move without breaking into some "nasty" eddies.

Professor Osborne Reynolds, by the use of a little coloured liquid, showed that the motion of the water at *very low speeds* was along A B and C D (fig. 27). This is what he would call "steady" or direct motion, but it comes under his rule No. 5—"a stream flowing through still water"—and this, as he most rightly says, is *conducive* to "unsteady motion." It is steady motion (in the sense in which he employs the word), but it is *unstable* motion, since a very slight increase of the velocity will cause the stream to break into sinuous motion or eddies.

If what has been said previously be accepted, the *pressure* in A B C D *must be less* than in the undisturbed water surrounding it. Why does not the undisturbed water flow into A B C D, *as it should*? It is evident that along A B and C D there is a surface of discontinuity, or, what is the same

thing, a free surface. This surface is capable of sustaining a certain amount of tension, and so is just able to prevent the flow which would otherwise take place. If the velocity of the stream be increased, the difference of pressures on the two sides of the film will become so great that the film will not be able to resist it: the film breaks and sinuous motion commences at once—*suddenly*. Doubtless the friction will *assist* in breaking this surface, but this result would occur even if there were no friction. Viscosity can, therefore, in no sense be said to be the *cause* of the eddies, though it may somewhat accelerate their production.

There appears no valid reason for supposing that a perfect liquid, if such a thing existed, would not behave in the same manner *in similar conditions*, provided its surface were capable of exerting tension.

SENSITIVE FLAME

The well-known "sensitive flame," so largely employed in acoustical experiments, will come under Osborne Reynolds' rule No. 5, and is a very good example of "unstable motion" (in the sense employed previously) kept "steady" by surface tension.

"If ordinary coal-gas stored in a gasometer is burnt at a small jet under considerable pressure, we are able to produce a tall flame about 18 to 24 inches in height. The jet used is one with a steatite top and a small pinhole gas exit about $\frac{1}{25}$ inch in diameter. [Professor Barrett said it was very essential that this aperture should be V-shaped: he recommends a $\frac{3}{8}$ -inch glass tube drawn out to $\frac{1}{16}$ inch.] The pressure of gas must be equal to about 10 inches of water, and it cannot be drawn straight off the house gas-pipes, but must be supplied from a special gasometer or gas-bag under a pressure sufficient to make a flame 10 inches or so in height. If the pressure is too great, the flame roars; if the pressure is slightly reduced, the flame can be made to burn quietly and form a tall reed-like flame (A, fig. 28). This flame, when properly adjusted, is curiously sensitive to shrill chirping

sounds. You may shout or talk loudly to it, and it takes no notice of the voice, but if you chirrup or whistle in a shrill tone, or clink your keys or clink a few coins in your hand, the flame at once shortens itself to about 6 or 7 inches in height, and exhibits a peculiarly ragged edge, whilst at the same time it roars (B, fig. 28). When in adjustment, the clink of a couple of coins will affect this sensitive flame on the other side of the room.

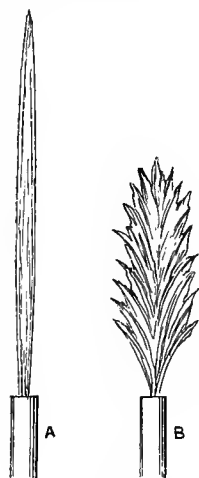


FIG. 28.

The flame is also very sensitive to a shrill whistle or bird-call. It will be clear to you, from previous explanations, that the flame responds, therefore, to very short air waves forming high notes. The particular flame I shall now use responds with great readiness to air waves of 1 inch to $\frac{1}{2}$ inch in length.

"It may be well to explain that the sensitive portion of the flame is in the root, just where it emerges from the burner; it is the action of the sound wave in throwing this portion of the flame into vibrations which is the cause of its curious action.

"If you think what the reaction must be, you will easily see that the operation of the sound wave is to throw the particles of the gas, just as they escape from the hole in the jet, into vibration in a direction transverse, or at right angles, to the direction of their movement in the flame. The gas molecules are, when unacted upon by the sound wave, rushing out of the jet in an upward direction. When the sound wave impinges on them they are, so to speak, caught, and caused to rock to and fro in a direction across the flame. The combination of these two motions results in a *spreading action of the flame*, so that instead of being a thin lance-like shape, it becomes more blunt, stumpy, and ragged at the edges" [italics added] (Dr Fleming, *Waves and Ripples*).

This description has been quoted at full length because it cannot well be improved upon; but the explanation is highly unconvincing. If the action is caused by the "rocking of the particles" of the flame at a definite *period*, depending on the

shrillness of the sound, and the longitudinal motion of the particles be *steady*, then the *combination of these two motions* should be a regular wave and not a ragged figure. We might put the following questions:—

1. Why is the same result not produced when a deeper note is sounded?

2. Why, if the flame is caused to rock, should it get *shorter*? There does not appear to be any “machinery” for shortening it.

3. Why should *exactly the same result* be produced when the particles are *not being rocked*?—i.e. when the *supply* of gas is increased. The rocking machinery does not exist in this case.

4. Why should the *length* of the flame be *decreased* when the *supply of gas is increased*? We might expect the very opposite result.

5. Why should “rocking the particles” cause the flame to *roar* and *become ragged*?

6. Why should the root of the flame be the most sensitive part? We should expect that the rocking action would be least apparent there, since the gas is moving fastest at this point.

The explanation breaks down under cross-examination. It appears to no more “explain” the action than if one were to say that pulling the trigger of a rifle *causes* the bullet to fly out of the muzzle. One follows from the other, but can hardly be said to be the *cause* of the other. You cannot say you *cause* a ball to fall to the ground *because* you let go of it. You *allow it to fall*.

I venture to suggest that the correct explanation is that the flame is at “full cock,” and the bird-call “pulls the trigger,” or “presses the button,” and, by so doing, allows the forces, all ready, to “do the rest.”

1. The pressure *inside* the flame is less than that *outside*.

2. The film round the surface of the flame *prevents* the air from rushing in.

3. The pressure of the gas is so regulated that the film is *just at the breaking point*.

4. A deep note throws the surface of the flame into *long waves* (fig. 29), thereby stretching the film, but only to such an extent that it is *just able* to bear the strain.

5. Short waves (fig. 29), caused by shrill notes, stretch the film much more: not being able to support this extra strain, the *film breaks*, and the air rushes in.

6. When the air rushes in, since violent eddies are produced, it is only natural to expect that the flame would be very

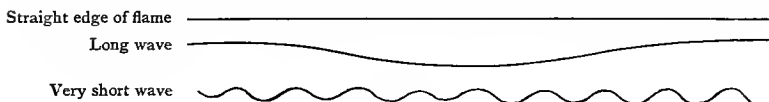


FIG. 29.

materially shortened; that the edges would be ragged; also that the flame would roar.

7. If the *supply of gas* be increased, the pressure inside the flame would be *reduced*, and this the film is unable to support.

8. The root of the flame is where the gas is moving fastest, and consequently where the difference of pressures on the two sides of the film would be the greatest. It would consequently be the *most sensitive part of the flame*.

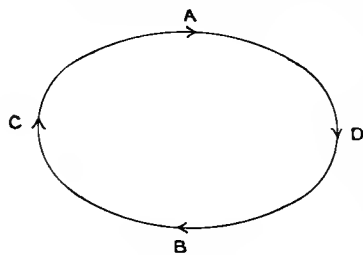


FIG. 30.

There is another peculiarity about these flames, which has been pointed out by Lord Rayleigh. That is that the flame is not symmetrically affected; or rather that it is

polar. If, for example, it is very sensitive from the side we may call N, it is also very sensitive on the side S; but much less so on the sides E and W. This fact has been known for a good many years, but Lord Rayleigh has never, that I am aware of, offered any explanation of it. The cause would appear to be that the particles of gas are issuing in a vortex which is not circular, but slightly elliptical. It will be evident that the flame (fig. 30) should be more sensitive in the direction C D than in the direction A B.

SINGING FLAME

Let us take another very similar case. Why does the well-known "singing flame" sing? The general arrangement is shown in the sketch (fig. 31); and the explanation is almost a repetition of what was said for the "sensitive flame," *with one exception*: the sensitive flame roars, whilst the singing flame is more genteel and "sings." What one may call the "trigger" of the arrangement is the air flowing up past the flame. Partly by the friction between this air and the flame, and partly by the difference of pressure, the film over the flame is broken and the flame gives a small roar. When this takes place the pressure is equalised across the tube and another film is formed, which is in its turn broken. This action going on very rapidly and periodically, a musical note is produced, and the glass tube acting as a resonator makes this audible. It is, in fact, a kind of very rapid making and breaking of the film, causing a periodic change of pressure in the tube, which causes the waves of sound. That the flame actually does go up and down—like the sensitive flame, only *periodically*—cannot, of course, be seen by the unaided eye: with the help of a rapidly revolving mirror, however, this is seen to be the case.

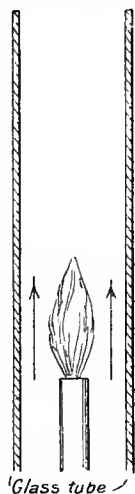


FIG. 31.

It may be argued that these two explanations are based on the *assumption* (with a very fair share of reason, it is hoped) that the pressure of the gas *inside* the flame was less than the pressure *outside*. Whether this is true or not can easily be decided by experiment. I regret that I have been unable to get any information on this subject about the sensitive flame. No one appears to know, and, apparently, no one has had the curiosity to try the experiment, which requires delicate apparatus. In the case of the "singing flame," however, thanks to the courtesy of Mr Leonard Bairstow of the National Physical Laboratory, I have been more successful. It appears that the pressure inside the gas flame was from $\frac{1}{1000}$

to $\frac{15}{1000}$ of an inch of water *measured negatively*. At these pressures the flame was only "humming." As the gas supply was *increased*, the humming got louder and the pressure, *measured negatively*, increased. All this thoroughly agrees with what one would expect.

When the resonator was removed, the pressure went down to $\frac{3}{1000}$ of an inch. The pressure of the gas at the main was, *roughly* (measured next day, only), *plus* $\frac{420}{1000}$ of an inch.

Since the apparatus for measuring the pressure *damps the vibrations*, what was really measured was only the *mean* pressure—say A B (fig. 32)—and not the *maxima* or *minima* of the curve (see sketch). The minimum would consequently

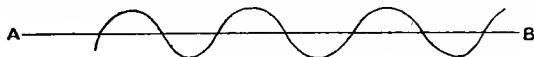


FIG. 32.

be less than even this (greater, if *measured negatively*). If the supply of gas had been increased, and the flame had been actually "singing," instead of humming, it is reasonable to suppose that the pressure, *measured negatively*, would have been still greater.

If the pressure inside a singing flame is less than the atmospheric, it is not, I hope, unreasonable to suppose that it would be the same in the sensitive flame.

In these cases viscosity has not been called in to produce the sinuous motion.

In the next chapter the motion of a liquid in a tube will be considered.

SUMMARY

For the formation of sinuous motion in a stream a cross pressure is necessary—a pressure across the line of motion of the stream.

The pressure inside the "sensitive" and "singing" flames is less than the atmospheric pressure.

REFERENCES

As before.

CHAPTER XI

LIQUIDS MOVING IN TUBES—POISEUILLE'S LAW— RING VORTICES

IN Professor Osborne Reynolds' list of "circumstances conducive to sinuous or unsteady motion," he gives (No. 6) "solid boundary walls." We will examine this next. If we imagine the sketch (fig. 33) to represent a solid boundary wall, *which is supposed to be perfectly wetted*, and a stream of water flowing past it. Since the wall is perfectly wetted, there will be a thin layer of water adhering to it. This water has been shown as *at rest*; but this may be taken as actually so, or, if moving at all, as moving so slowly that it may be regarded as at rest relatively to the boundary wall. At fairly high velocities there will be a free surface between this layer and the moving water. From what has been said previously, it will be evident that the water "at rest" is under greater pressure than the moving water and that a flow will take place as shown. Eddies or sinuous motion are represented as starting.

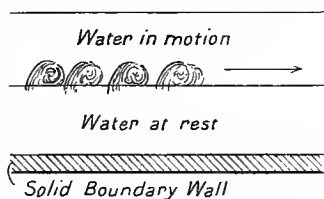


FIG. 33.

If we next imagine the boundary wall to be a small piece of a tube, the action will, of course, be the same, and we can, in imagination, see the water in the tube breaking into small whirls or eddies. Since the formation of these causes *inertia* resistance, the expression for R changes from $R = AV$ to $R = AV + BV^2$.

As previously pointed out,¹ Dr Young appears to have been the first to state that the resistance "could not be represented by any single power of the velocity," but that "it appeared to consist of two parts, the one varying simply as the velocity, the other as the square." His formula was—

$$f = a \frac{l}{d} v^2 + 2c \frac{l}{d} v.$$

Girard made very many experiments and found the same thing. His *formule générale* was—

$$g \frac{Dh}{4l} = au + bu^2,$$

where "le premier terme *au*, de la force retardatrice, représente la portion de la résistance qui est *due à la cohésion des conches fluides entre elles* [italics added] tandis que le deuxième terme *bu*² représente la portion de la résistance qui est produite par les aspérités de la surface sur laquelle le fluide se meut" (*Institut de France*, 1816).

So many other writers in the early part of the nineteenth century appear to have arrived at the same conclusion, that Poncelet et Lesbros (Capitaines du Génie) in 1832 wrote (*Savans Étranger*, vol. iii.): "Le mouvement uniforme de l'eau dans les canaux et les tuyaux de conduite réguliers, d'une grande longueur, a pu être soumis au calcul dans ces derniers temps, de la manière *la plus heureuse et la plus satisfaisante*, par MM. Girard, de Prony, Navier et Etylwein; les formules ainsi obtenues ne semblent plus *rien laisser à désirer* du côté de *l'exactitude des applications*" [italics added].

It will not appear necessary for me to labour this point any more.

It may occur to anyone who reflects on the matter that since the water in a lead pipe is in a violent state of turmoil, caused by the eddies and rotational movement, it very naturally issues from the tap somewhat as in sketch A (fig. 34). The water is not acting at all like a liquid, but as a solid, or series of solid particles. The particles strike any object

¹ Chap. V. p. 35.

they meet with considerable violence and *rebound* from it, causing what is commonly called "splashing." Possibly some of the spreading of the particles may be due to electrical conditions, but it is chiefly caused by the particles of water striking against one another.

Now, if we could, by some means or other, "comb all the curl" out of this water and cause each small filament to issue *at the same velocity*, the result should be quite different and more in accord with the stream-line theory. This is

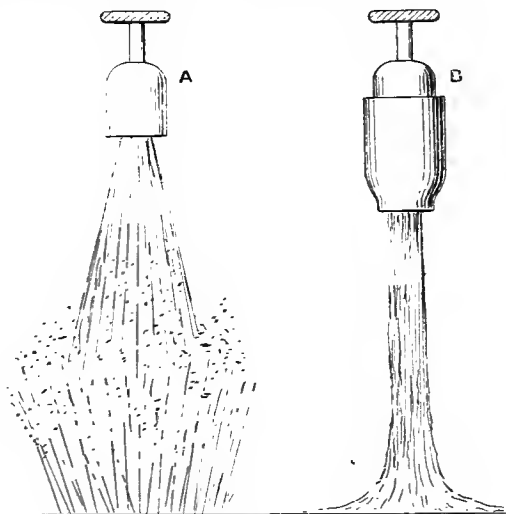


FIG. 34.

done daily in many a scullery by means of the well-known "anti-splash" tap. The essentials of this tap are that the water is caused to pass through a very fine screen with a piece of sponge above it. As is well known, the water issuing from this tap has the appearance of a glass rod, and when it falls on any object it *does not splash* (fig. 34, B).

POISEUILLE'S LAW

So far we have only considered liquids flowing in ordinary tubes, at ordinary speeds. When the tubes are very fine, or the velocity of flow exceedingly small, then the resistance

varies as the velocity only. In these cases there is no "discontinuity" in the liquid, and the flow may be considered as due to the motion of an infinite number of infinitely thin co-axial cylindrical elements, the outermost of which is at rest, in contact with the inner surface of the tube; the next moving with small velocity inside this one; the next with greater velocity inside the last, and so on to the axis, where the velocity is a *maximum*.

In the middle of the nineteenth century, Poiseuille, who was a doctor, being desirous of finding out the velocity of flow in the small veins of animals, made a series of most remarkable experiments on the flow of water in fine glass tubes, which he published in the *Comptes Rendus*, vols. xi. and xii. With tubes of about .14 millimetre he found that with a constant pressure the discharge varied as the *fourth power of the diameter* and *inversely as the length* of the tube. This is now known as "Poiseuille's law." Poiseuille also put the law in another way: "La vitesse dans les tubes de très-petit diamètre est proportionnelle à la pression, en raison inverse de leurs longueurs, et proportionnelle au carré de leurs diamètres" (Poiseuille, *Comptes Rendus*, xi.).

On the assumption of the flow made above, it is not difficult to show that, theoretically, the *mean* velocity of the liquid in the tube must vary as the *square of the diameter* of the tube; and consequently the discharge will be as the *fourth power* of the diameter, which was what Poiseuille found experimentally. He points out another curious thing, and that is, that whilst the tube was 51 mm. in length, the relation between time of discharge and pressures always held good; but when it was reduced in length to 25.55 mm., 15.75 mm., 9.55 mm., and 6.75 mm., "cette relation n'a plus lieu; les temps, pour des pressions de plus en plus considérables, *sont plus grands* que ceux que donnerait la relation dont il est question" (Poiseuille). That is to say, the resistance is *greater* than it ought to be according to his law. The reason for this is, of course, that the velocity, exceeding the "critical," discontinuity has commenced and, *ergo*, sinuous motion. We have therefore the curious fact

that the resistance *decreases as the tube is shortened*; but at a certain length the resistance *suddenly increases*, as we should expect. He also points out that the addition of iodide of potassium reduces the resistance—and consequently time of flow of a fixed quantity of liquid—*very considerably*. This is true for all amounts even up to one-third salt and two-thirds water (*by weight*). Many other salts act in the same manner, viz., bromide of potassium, nitrates of potassium and ammonium, chlorides of ammonium and potassium, cyanide of potassium, and acetate of ammonium. Other salts act in the opposite manner, viz., nitrate of sodium, chloride of sodium, sulphates of potassium and ammonium, and phosphates of potassium and ammonium. Some salts act in one way at certain temperatures and in the opposite manner at other temperatures. It is ordinarily taught that these salts increase or decrease *the viscosity*. It is difficult to believe that a very syrupy liquid is *less viscous* than water. It appears more probable that the liquids have their *adhesion* to the glass decreased or increased.

In Chapter IX. the question of a liquid slipping on a boundary wall was discussed, and it was said that this would be referred to again after the explanation of Poiseuille's law. I quoted Professor Lamb as having stated that this law of Poiseuille furnished "*conclusive proof*" that the liquid did not slip "to any appreciable extent," and I gave reasons showing that another explanation of the facts appeared to me to be equally satisfactory. The subject will here be considered in another manner.

Poiseuille's law is that the discharge through a small tube at constant pressure $= A \frac{d^4}{l}$. This, however, is not the whole law, for there are certain corrections required for temperature, and when these are added the formula becomes

$$D = A \frac{d^4}{l} (1 + \alpha T + \beta T^2),$$

where T = temperature in degrees Centigrade, and α and β are constants.

If there were no slipping, the law connecting viscosity and temperature ought to be expressed by

$$\mu_T = \mu_0(1 + \alpha T + \beta T^2)^{-1},$$

where μ_T is the coefficient of viscosity at temperature T (Cent.), which does not appear to be the case.

"O. E. Meyer finds that for a considerable number of liquids the simpler formula $\eta_t = \eta_0(1 + \alpha t)^{-1}$ answers well enough for all practical purposes" [η is the coefficient of viscosity] (Castell-Evans' Physico-chemical Tables).

An examination of a list of the viscosities of over a hundred different liquids at different temperatures (made by Rellstab and Pribram, and Handl) shows that this law of Meyer's is *exactly* true for all temperatures from 10° C. to 50° C. in the case of the following liquids:—acetone, ethyl ether, methyl acetate, methyl alcohol, methyl butyrate, methyl valerate, propyl acetate (*n*), propyl (*i*) acetate, propyl (*n*) iodide, propyl (*n*) nitrite, propyl (*i*) propionate, and valeral.

If we consider the effect caused by the attraction of the different layers of the liquid to the tube, these peculiarities appear to me to be capable of a satisfactory explanation.

Heat appears to act on the liquid in two different ways:—

(1) It reduces the viscosity, or treaciness, of the liquid, and so the flow is increased by some small fraction.

(2) It reduces the attraction of the liquid to the walls of the tube, and so increases the *effective* bore of the tube, either by allowing the liquid to flow more freely through the tube, or by allowing the liquid to slip along the walls.

The law of (1) is not very exactly known, but there appear to be very fair reasons for supposing that it is something like $\mu_T = \mu_0(1 + \alpha T)^{-1}$, when the discharge would increase as some function of the *first power* of the temperature.

Since in (2) the temperature acts by practically enlarging the effective bore of the tube, if we put this enlargement of the diameter as $(1 + \beta T)$, the discharge will be increased by some function of the *square* of the temperature, something like

$$D = A \frac{d^4}{l} (1 + \beta T)^2,$$

and if both actions be taken into account the formula should be

$$D = \frac{Ad^4}{l}(1 + \alpha T)(1 + \beta T)^2,$$

or

$$= \frac{Ad^4}{l}(1 + \alpha T + \alpha' T^2 + \alpha'' T^3).$$

The term $\alpha'' T^3$ is so small as to be negligible, so (following Poiseuille) if we omit this we get

$$D = A \frac{d^4}{l}(1 + \alpha T + \alpha' T^2).$$

It would appear, therefore, that the rational expression of Poiseuille's formula should be

$$D = A \frac{d^4}{l}(1 + \alpha T)(1 + \beta T)^2,$$

where α and β are not *necessarily* interdependent.

As the matter is important I will put the case in another way and apply a crucial test. It is quite commonly stated that the viscosity of alcohol is *very much* greater than that of water. This is not strictly correct. What is *meant* by the statement is, that alcohol *and water* flows less freely through a tube than water does. Dubuat was most cautious in his statement of this fact: "L'esprit de vin coule *sensiblement* moins vite que l'eau à cause de sa viscosité, *ou de sa plus grande adhérence aux parois*" [emphasis added]. The truth is that the coefficients of viscosity of water and (pure) alcohol are *almost exactly the same*. The "most probable value" of that of water, at 0° C., being .0181456, whilst the coefficient for alcohol, at the same temperature, is .018430, the difference is not *very great*. If, however, you mix water and alcohol, you get a liquid which flows *much more slowly* through a fine tube, and is therefore said to have *much greater viscosity* (?). If the mixture contains 35.11 per cent. of alcohol, the coefficient of viscosity (?) is *more than three times as great*, viz., .05703 at 10° C.; and probably greater than this at 0° C. By adding more alcohol and making the proportion 49 per cent., the viscosity (?) falls

to '04133 at 10° C. At 70 per cent. it is still lower, being only '03279 at the same temperature. "Traube found in every case [? at all temperatures] the *maximum* viscosity fell between 40 and 50 per cent. (inclusive)" (Castell-Evans' Physico-chemical Tables).

It is difficult to believe that by mixing two liquids of small viscosity you will produce a liquid which has such an enormous increase of viscosity—cases where chemical combination takes place, must, of course, be excluded. The correct explanation would appear to be that the mixture has much greater attraction to the glass and so "chokes" the tube.

It may be thought that this is a case of substituting one improbability for another. That is not so. It is well known that if you mix alcohol and water they contract: the molecules are closer together and their attraction to the walls of the tube might be *expected* to increase. If this be the true explanation, then the *maximum* attraction to the glass should correspond with this *maximum contraction*; and this is found to be the case. The *maximum* viscosity (?) is found in a liquid containing nearly 50 per cent. of alcohol; whilst the *maximum* contraction is found in a liquid containing 52 per cent. of alcohol. The agreement is very close for a first approximation and the *relation is regular*. If you add alcohol to water the viscosity (?) gradually increases until the mixture contains 52 per cent. of alcohol; on continuing the addition of alcohol the viscosity (?) decreases with the increased proportion of the spirit. It appears to be a curious fact that there is more contraction when you add *water* to *alcohol* than if you add *alcohol* to *water*. For example, if you add 10 parts of water to 90 parts of alcohol, the contraction *exceeds* 1·75 per cent.; whilst if you add 10 parts of alcohol to 90 parts of water the contraction is only '714 per cent., or considerably less than half that in the other case.

A short time ago a chemist friend very kindly took the flow of absolute and 50 per cent. alcohol through a Boverton Redwood viscometer. He could find *no appreciable difference*! and yet the latter is supposed to have between *three and four times more viscosity*.

Take another case. Fill a V-tube (fig. 35) about two-thirds full of water and boil it well, sealing up the tube whilst the water is boiling freely. Putting one arm horizontally and carefully tapping the tube—to allow the water to settle well down in it—you can gently raise this arm, as in the sketch, and the water will not flow out of it. Now, how is that water kept up, except by its “stiction” to the tube? (I must apologise for this word, but it expresses forcibly what I mean.) If the water is kept up by viscosity, and if it cannot slip on the walls of the tube, why should the water, if the tube is tapped, fall with great velocity?

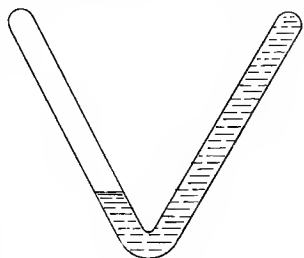


FIG. 35.

It is not pretended that the foregoing is an absolute *proof* that a liquid may slip along the walls of a tube, but I certainly think that it points to the *probability* that it does so in some cases—and that the effect may be completely masked.

In any case, it shows that the subject of viscosity requires more work being done upon it: a new method of measuring viscosity is much wanted. The view I have advanced gives a rational explanation of the empirical law of Poiseuille regulating flow through small tubes and temperature.

RING VORTICES

Let us now examine what should be the motion of a liquid behind a thin lamina, which we will suppose to have rounded edges. If A B (fig. 36) be a section of the lamina, the water flowing past it will proceed at first *in the directions* A E and B F. This may be taken as a case of Professor Osborne Reynolds (No. 8): “Circumstances conducive to unsteady motion”—viz., “Curvature with the velocity greatest on the inside.” At any reasonable velocity the liquid will be “cracked,” and a surface of discontinuity formed *in the directions* A E and B F. It is not important to consider *how far* this surface extends, nor is it necessary for my present purpose; it is sufficient that

it is formed. This free surface has, as usual, a film which is capable of sustaining tension. At *very low speeds* we might *imagine* the crack as extending for some distance—as in Dr Hele-Shaw's experiment quoted previously; at any ordinary speeds, however, the film would break at once. Now, if we suppose that when this takes place the water in the rear of the lamina is *at rest*; the pressure at C being very much greater than at A and B, a flow of liquid will commence outwards, as shown by the arrows. The pressure at C will then be reduced and a flow will commence from D to C.

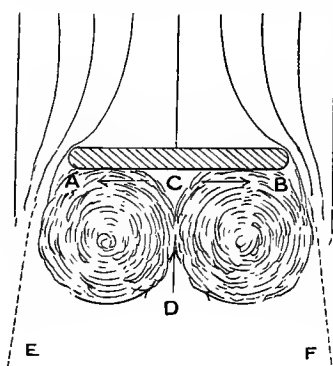


FIG. 36.

Similarly, a flow towards D, as shown by the arrows, and eventually a complete ring vortex will be produced, without calling in the aid of viscosity or liquid friction.

The action has been described, for simplicity, as taking place step by step. The vortex would, however, be formed by all these flows taking place at the same time, or, at least, in the most rapid succession. It will be evident that since this ring vortex has been produced *without the assistance of viscosity* that it could equally well be formed in water or in a "perfect" liquid. In the latter case it would *necessarily* be an *irrotational vortex*, and consequently a coreless vortex. In water it would not necessarily be an irrotational vortex, but it would equally be a coreless one. Further reasons for this last remark will be given later.

There are four objections which may be, very reasonably, raised against this explanation.

1. No evidence has been produced that such a vortex actually *is* formed behind a lamina: ordinarily one sees nothing but a confused mass of eddies.

2. If a vortex *were* formed, since, practically, no work would be required to keep it rotating, all resistance should cease when the vortex was once formed.

3. As a ring vortex is a complete entity (acting, as it does, like an elastic solid), the pressure on the *rear of the lamina* would be that transmitted, *from the liquid in rear, through the vortex*. Such pressure would be the hydrostatic pressure; and, since it has been shown that the pressure on the *front of the lamina* is *less than this*, the lamina should *move forwards—against the stream*.

4. That the formation of a vortex in a “perfect” liquid would be equivalent to an act of creation; that therefore no vortex could be produced; and, equally, if a vortex existed, it would be *impossible to destroy it*.

These objections are all perfectly reasonable and will require to be dealt with separately.

To consider No. 1 first. *Can* a vortex be formed behind a lamina? An experiment of Osborne Reynolds was described previously, where a small disc was suspended from a float. When this was given a *sudden* push, and was released the combination travelled along *as if meeting with no resistance*. Further, if this combination were *stopped, and released instantly*, it would *restart itself*. It was remarked that what was taking place would be considered later. It will now be evident that a vortex had been formed, and that this vortex will explain what appeared mysterious. That this is so can be easily made evident by placing a small quantity of coloured liquid immediately in front of the lamina, before moving it, when a very beautiful ring vortex will be made evident by the coloured liquid which, owing to the motion of the lamina, is now behind it.

Professor Osborne Reynolds describes this vortex as “bearing something the same proportion to the visible ring as a ball made by wrapping string (in and out) round a circular curtain ring until the aperture was entirely filled up.

“The disc, when it was there, formed the front of this ball or spheroid of water, but the rest of the surface of the ball had nothing to separate it from the surrounding water but its own integrity. Yet when the motion was very steady, the surface of the ball was definite, and the entire moving mass might be rendered visible by colour. The water within

the ball was everywhere gyrating round the central ring, as if the coils of string were each spinning round the curtain ring as an axis, the water moving forwards through the interior of the ring and backwards round the outside, the velocity of gyration gradually diminishing as the distance from the central ring is increased.

"The way in which the water moves to let the ball pass can also be seen, either by streaking the water with colour or suspending small balls in it. In moving to get out of the way and let the ball of water pass, the surrounding water partakes, as it were, of the gyratory motion of the water within the *ball*, the particles moving in a horse-shoe fashion, so that *at the actual surface of the ball* the motion of the water outside is *identical with that within*, and *there was no rubbing at the surface and consequently no friction*.

.

"It is impossible to have a ring in which the *gyratory motion* is *great* and the *velocity of progression* *slow*. As the one motion dies out so does the other, and any attempt to accelerate the velocity of the ring by urging forward the disc invariably destroys it" (Royal Institution, February 1877).

I have quoted this description very fully, as it gives a most vivid and graphic picture of what takes place behind the disc. The explanation which follows, however, hardly seems to me quite satisfying.

"The striking ease with which the vortex ring, or the disc with the vortex ring behind it, moves through the water naturally raised the question as to why a solid should experience resistance. Could it be that there was something in the particular spheroidal shape of these balls of water which allowed them to move freely? To try this, a solid of the same shape as the fluid ball was constructed and floated after the same manner as the disc. But when this was set in motion, it stopped directly—it would not move at all. What was the cause of this resistance? Here were two objects of the same shape and weight, the one of which moved freely through the water, and the other experienced

very great resistance. As already explained, there is no friction at the surface of the water, whereas there must be friction between the water and the solid. But it could be easily shown that the resistance of the solid is *much greater than can be accounted for by its surface friction or skin resistance*. The only [?] other respect in which these two surfaces differ is that the one is flexible while the other is rigid, and *this seems to be the cause of the difference in resistance*.

“Colouring the water behind the solid shows that instead of passing through the water without disturbing it, there is a very great disturbance in its wake” (Royal Institution, February 1877).

As stated previously, this explanation is hardly convincing. I will endeavour to give what I think is the true explanation later, after discussing “Helmholtz rings.”

We have now data to enable us to reply to objections 1, 2, and 3.

1. Vortices *are* formed behind a circular lamina.
2. When the vortex is formed, the resistance *actually does cease*, the disc moving with *no appearance of resistance*. This is only half the truth, however, for “if the speed of the disc *were maintained uniform*, the ring gradually dropped behind and broke up” (Osborne Reynolds). When, therefore, the disc is fixed and the stream moves *uniformly* past it, or if the disc be moved *uniformly* through the water, the vortices are being perpetually “sloughed off.” Other vortices are then formed, to be sloughed off in their turn, and so on. It is this continual *formation* of vortices which causes the *inertia* resistance, and the breaking of these which causes the appearance of “eddies,” or the confused turmoil of the water.

We may imagine the action of “sloughing off” the vortices to be somewhat as follows:—The disc commences to move a “little too fast” for the vortex, which consequently lags behind slightly. A fresh crack is formed in the liquid at A and

B (fig. 37), just in front of the vortex. A new vortex is formed in the liquid, as shown in the sketch, and this "peels" the old one off the disc, destroying it in the process: just as you can peel off soap-bubbles.

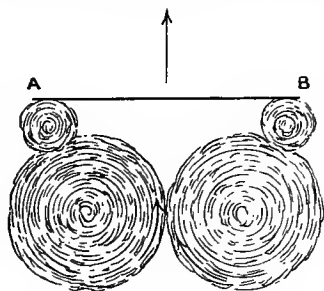


FIG. 37.

It must be remembered that though this vortex may be said (popularly) to be able to propel itself, it will only do so *at its own pace*, which is not, necessarily, the pace *you wish it to move at*.

These three objections may therefore be considered as having been satisfactorily answered. The question of the formation of vortices in a perfect liquid will be dealt with in the next chapter.

SUMMARY

Liquids flowing in tubes are subject to the same law of resistance $R = AV + BV^2$. If the tubes are *very small* (capillary), or the velocity of flow is *very low*, then "inertia resistance" (dynamic resistance) does not occur, and the resistance may be expressed as $R = AV$ only.

In the case of capillary tubes the discharge of liquid taking place through them (with a fixed pressure) varies as the *fourth power* of the *diameters* of the tubes and *inversely* as their length.

Some salts added to water cause it to flow more freely through the tubes: other salts act in exactly the opposite manner.

When a circular disc moves in water ring vortices are formed behind it. These are only stable if the disc is moved with a "staccato movement" and then released. They will only persist if *allowed to move at their own speed*.

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CHAPTER XII

VORTICES IN A PERFECT LIQUID—HELMHOLTZ RINGS

JUST as it is commonly taught that a body moving in a perfect liquid would meet with no resistance, so, and probably for the same reasons, it is taught that it would be impossible to produce any vortices in a perfect liquid. Dr Fleming is quite clear on this point, for he says (*Waves and Ripples*): "If they [eddies] were created [in a perfect fluid], they would continue for ever, and have something of the permanence of material substances."

This was, for many years, the view held by Lord Kelvin, for in 1869 he wrote: "The simple circular Helmholtz ring is a case of stable steady motion, with energy maximum—minimum for given vorticity and given impulse. A circular vortex ring, with an inner irrotational annular core, surrounded by a rotationally moving annular shell (or endless tube), with irrotational circulation outside all, is a case of motion which is steady, if the outer and inner contours of the irrotational shell are properly shaped, but certainly unstable" if the shell be too thin. It was essentially on this view that he framed his famous theory of matter. In 1875 he still held the view that to make a vortex in an inviscid fluid would be equivalent to an act of creation; but in the *Phil. Mag.*, 1880, he admits that "hitherto I have not indeed succeeded in rigorously demonstrating the stability of the Helmholtz ring *in any case*" [italics added]. In 1887 he apparently changed his views, for he published his classical paper "On the Formation of Coreless Vortices by the Motion of a Solid through an Inviscid Incompressible Fluid" (*Phil. Mag.*, vol. xxiii.). Finally,

in 1904 (*Waves in Water*), he frankly says: "After many years of failure to prove that the motion in the ordinary Helmholtz circular ring is stable, I came to the conclusion that it is essentially unstable, and that its fate must be to become dissipated as now described." He had finally, with great regret, abandoned his vortex theory of matter.

It is not possible to experiment with a perfect liquid; so that what it would or would not do is a good deal a matter of opinion—or perhaps, more correctly, it depends on the properties you postulate. In all that has been said here about the vortex ring, it will be noticed that viscosity has been ignored. The vortex was not considered as (1) *formed* by the aid of viscosity; (2) *retarded* by viscosity, there being no friction between the water flowing round it and the vortex; and (3) it was not dissipated by viscosity. There appears, therefore, no reason why similar vortices should not be formed, in a similar manner, in a perfect liquid, if such a thing existed. Of course the perfect liquid must possess *all the properties of an ordinary liquid*, with the single exception that its particles *cannot be rotated*. Without this assumption the perfect liquid could only be considered by engineers a "curiosity," the most suitable place for which would be a Mathematical Museum.

HELMHOLTZ RINGS

These rings are probably about the easiest form of vortex to experiment with, and are commonly known as "smoke rings." They can be seen occasionally coming out of the funnel of a locomotive, out of the mouth of a smoker or the muzzles of heavy guns—the old 40 pr. Armstrong sometimes produced very fine ones of a lemon-chrome tint. As is well known, they are most commonly formed by means of a box with a round hole in front and an elastic back (fig. 38). On filling the box with smoke and striking A B sharply, a smoke ring issues from C D, as in the sketch.

Dr Fleming (*Waves and Ripples*) describes the action very graphically: "The motion of the air or smoke particles composing the ring is like that of an indiarubber umbrella ring

fitted tightly on a round ruler and pushed along." It must be remembered, however, that, though this gives a most graphic *idea* of the motion, it is in reality *exactly the reverse*. The motion described would be as at A (fig. 39), which represents a central section of an umbrella ring perpendicular to its plane, whereas the real movement is as in B (fig. 39). If it were as in A, the ring would not travel forwards (as it does), but would tend to go backwards: it is im-

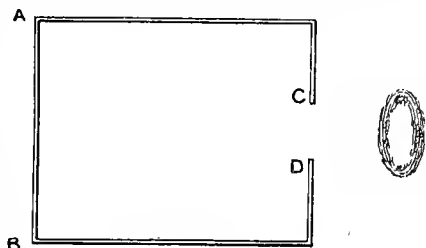


FIG. 38.

portant to remember that this ring vortex propels itself. How this is effected will be considered later.

How is this smoke ring, or ring vortex, formed? Dr Fleming says: "This rotary motion is set up *by the friction* of the smoky air against the hole in the box, as the puff of air emerges from it when the back of the box is thumped."

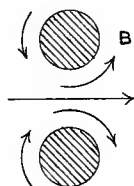
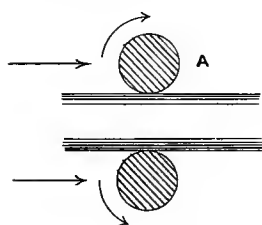


FIG. 39.

This description is not very full, but the impression conveyed by it is that (1) a couple is formed by the friction of the moving air, and (2) that the smoke ring is formed *in* the opening

of the box. It is not difficult to show that the smoke ring is not formed in this opening, but outside the box altogether.

Now, if the vortical motion were caused *by the friction* of the air against the hole in the box, we should expect that the greater the friction the better would be the vortex, and *vice versa*. Such is not the case, for the very best rings can be produced when the side of the box is very thin and the hole has *very sharp edges*. In this case the friction would be

quite insignificant. Reusch has also shown (*Pogg. Ann.*, cx.) that if a tube, or nozzle, be fixed to the hole in the box, the ring formation goes on very well until the length of the tube is five times the diameter—then *the rings are not formed at*

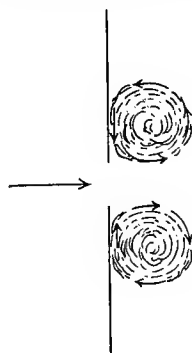


FIG. 40.

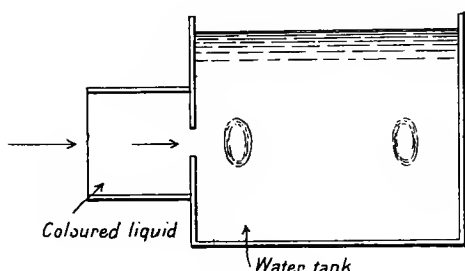


FIG. 41.

all. In this case we have very great increase of friction, but no smoke ring.

The correct explanation would appear to be that the ring is formed *outside* the box and clear of the opening. The sketch (fig. 40) and what was said about the formation of the

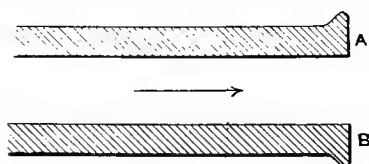


FIG. 42.

vortex behind the lamina will explain clearly the action in forming these "smoke rings."

The reference has been made to these Helmholtz rings in *air*, because the experiment can be so easily

repeated. Professor Osborne Reynolds produced them in a tank of water by means of the arrangement in the sketch (fig. 41). They behave exactly as do the smoke rings, though they do not travel so fast.

Probably the foregoing is the reason why heavy guns are corroded at A and B (fig. 42) after much firing.

In the formation of smoke rings, by means of a smoke box, it is essential that a sudden blow be given to the back of the box, so as to cause discontinuity of the fluid. It is not

specially necessary that the opening should be very round. Reusch has shown that circular rings can be produced when the opening is triangular, square, or even a *not over-long* rectangle. With a rectangular opening of 2:1 round rings are always produced; but if the proportion of the sides be 4 or 5:1, then two rings are formed. When the proportion reaches 6 or 7:1, with care, three rings can be produced, the central ring being frequently crippled.

When the smoke ring has been formed and is moving away, the elastic back of the box swinging back causes a "rarefaction" in the box, and air rushes in from the outside. Another surface of discontinuity is formed *inside the box*, and an "air ring" is formed which travels *backwards in the box*. We have thus a "smoke ring" travelling in clean air away from the box; whilst a clean "air ring" travels through the smoky air in the opposite direction. This very curious fact, which was first pointed out by Reusch, can easily be seen if the box is transparent and the smoke *not too dense*. No English authors, that I am aware of, appear to have observed this.

When you look at an ordinary smoke ring, you really only see part of it—what I may call its skeleton. The outer part, being composed of clean air, is not visible. If you put smoke *outside* the box (in front of the hole) *instead of inside*, then quite a different view will be obtained. The *ring* will not be seen, but the outer part of the vortex will; and this will show that it is spheroidal in shape, reminding one somewhat of the dandelion seed. This very curious fact was apparently first pointed out by Sir Robert Ball (Royal Dublin Society) in May 1868. He caused "air rings" (smoke rings without smoke) to issue from a "smoke box," then to *pass through smoke, outside the box*. He described the result as follows:—"The air ring had penetrated the smoke uninjured; it had not apparently left any of its particles behind, nor had it admitted an atom of smoke into it; but it had *drawn with it* sufficient of the smoke to *form a complete shell*, which enclosed it, and *thus rendered the air visible* [*italics added*]. . . . The appearance is one of great beauty and suggested the name 'negative

smoke ring.'"¹ It will now be apparent that there is no *essential* difference between the small vortex formed behind Osborne Reynolds' disc and the vortex formed by what Reusch calls the "staccato movement" of a fluid through a circular opening. Both are "spherical vortices."

Sir Robert Ball measured the velocity of these air rings at different parts of their flight, and found that the velocity increased until they had got about 4 or 5 feet from the smoke box; the speed then remained fairly constant for some distance, after which it decreased. Osborne Reynolds found practically the same thing when he made these rings in water, for he says: "It is found that the force of the blow they will strike is nearly independent of the distance of the object struck from the orifice."

I am not aware of any explanation having been offered of this curious motion, so I venture the following.

If we refer to the sketch of the vortex formed behind the lamina and remember what was said about its formation, it will be evident that cyclic energy has been generated behind the disc. Since it is impossible to create, or destroy, energy; this energy can only have been generated at the expense of the kinetic energy of the stream-lines; their $\frac{1}{2}\rho v^2$ has been reduced, and they will consequently be travelling *more slowly round the rear of the vortex* than they did in front of it. They will therefore *exert more pressure there*. Then, since the vortex is a complete entity, with greater pressure behind it than in front, it will travel forwards with a gradually accelerating velocity. The vortex travels faster and faster for a certain distance and then the velocity becomes steady. All this while, it may be said, familiarly, that the vortex is *sucking kinetic energy* out of the stream-line. Whilst it is doing this to *increase its velocity*, it has also been absorbing fluid from the stream-line so as to *increase its size*. These two actions are opposed to one another. At first the velocity increases, but as the size also increases, after a certain time the acceleration

¹ The term "negative smoke ring" would appear to be more suitable for Reusch's rings, which are really rings, and *which travel in the opposite direction* to the ordinary ring.

ceases, and the velocity remains constant; ultimately the vortex gets "too fat" and the cyclic velocity *diminishes*. Remembering Professor Osborne Reynolds' statement of the connection between the gyratory motion and the velocity of progression, it is evident that as the gyratory motion decreases so will the velocity of progression, and the vortex will eventually be dissipated. We now see why the disc in Osborne Reynolds' experiment, previously referred to, when stopped and *released instantly*, starts itself again.

That these vortices increase in size is confirmed by Osborne Reynolds, when referring to those made in water: "Their velocity gradually diminishes; but this would appear to be accounted for by their growth in size, for they are thus continually taking up fresh water into their constitution, with which they have to share their velocity."

The question of the *smoke* ring having a maximum velocity has been specially referred to, as Professor Osborne Reynolds (who is generally right) says, in reference to ring vortices in water: "These rings are much more definite than smoke rings, and although they cannot move with higher velocities, *since that of the smoke ring is unlimited . . .*" This cannot be admitted, and it certainly does not appear to be confirmed by experiment. It must be remembered that this statement was made in 1877—a *very long time ago*. If the explanation of the formation of the vortex be correct, it will be evident that the strength of the vortex is regulated by the pressure in the still water. As this is fixed by the depth of immersion + atmospheric pressure, it will be obvious that there is a *maximum* gyratory motion, and, *ergo*, progressive motion—which *cannot possibly be exceeded*. We might indeed imagine an immersion of *infinite depth*; but—and it is a very big but—at this immersion a crack in the fluid could not be made, and the *vortex could never be formed*.

These vortices are always coreless. It is difficult to produce actual proof of this; but here again an experiment of Osborne Reynolds supports the statement: "Yet a still more striking spectacle may be shown, if, instead of coloured water, a few bubbles of air be injected into the box from which the puff is

sent; a *beautiful ring of air* is seen to shoot along through the water. . . ." (Royal Institution, 1877). This "ring of air" would appear to be the core—vacant of water—of the vortex ring.

There is another argument from a theoretical point of view. I have been informed by a professor, who has made a speciality of vortices, that an ordinary vortex (in the strict mathematical sense) *can be cut across*, whilst a coreless vortex *cannot*. It is very well known that you cannot cut a "smoke ring." In any case it appears very fairly certain that *no crack in fluid, no vortex; and no vortex, no inertia resistance*.

The whole subject of smoke rings is a most fascinating one. To watch the first stages of the motion, when at *very low velocity* the smoke emerges from the hole in the form of a mushroom; how this mushroom develops into a sort of "ionic capital"; to the perfect smoke ring, when the fluid is cracked by a sudden blow on the box: all this is most beautiful. The reader must, however, be referred to Reusch, as well as Rogers (*American Journal of Science and Arts*, vol. xxvi., 1858), for further details.

I cannot do better, in closing this chapter, than finish with a quotation from Professor Osborne Reynolds:

"That the vortex takes a systematic part in almost every form of fluid motion was now evident. Any irregular solid moving through the water must from its angles send off lines of vortices such as those behind the oblique vane. As we move about we must be continually causing vortex rings and vortex bands in the air. Most of these will probably be irregular, and resemble more the curls in a smoke cloud than systematic rings. But from our mouths as we talk we must produce numberless rings" (Royal Institution, 1877).

In this chapter the words "vortex" and "ring vortex" have been employed. What was meant will, I trust, have been fairly evident; but to prevent any misunderstanding, it may be well to be precise and to give a little more explanation, especially as "coreless vortices" are not well understood and

they differ from the ordinary mathematicians' vortex referred to in their treatises. To begin with the latter:—Let us imagine the sketch (fig. 43) to represent, diagrammatically, the section of a vortex and of its liquid cylinders.

"The velocity of the fluid is everywhere inversely as the length of its path of flow; consequently, if we suppose the cylinder be made smaller, the velocity at its surface will be proportionally greater, so that in the limit, if we suppose

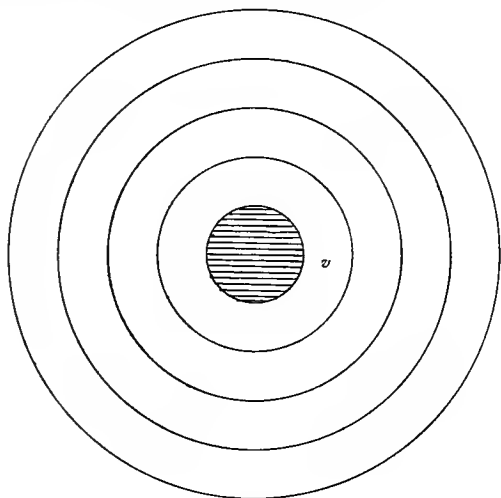


FIG. 43.

the cylinder to become evanescent, the velocity becomes infinite."

"Such a motion is known as *vortex motion*, and the system figured constitutes a *vortex filament*. It will be seen that if r represents the radius of the path of flow and v the corresponding velocity, $vr = \text{constant}$, and if the angular velocity $\omega = v/r$, we have $\omega r^2 = \text{constant}$ —that is to say, for any circuit of flow the area \times angular velocity is constant, which is the relation for vortex motion established *generally* by the theorem of Helmholtz and Kelvin" (Lanchester, *Aerodynamics*).

This excellent description of Mr Lanchester's gives a very

clear idea of the mathematicians' vortex, which, however, differs in some ways from the vortices which I have described as being formed in water or air. In these latter the velocity v is *strictly limited by the energy* (potential+kinetic) of the stream-line. Since $vr=\text{constant}$, it also follows that r is limited. If we imagine v , in the diagram (fig. 43), to represent the section of the cylinder having the maximum velocity attainable, all the shaded part, inside v , will become void of water and be a "hollow core." This is not necessarily a *vacuum*, since it may be filled with air, or water vapour—the essential is that it is *void of water*: in the "smoke ring" it is full of smoke. All the remainder of the vortex will, of course, obey the law $vr=\text{constant}$. This will, I hope, give a clear idea of what I mean when I speak of a "coreless vortex."

The mathematicians' vortex is *imaginable*; but it would, I think, be beyond the power of human imagination to conceive how it could be formed. It would require the consumption of *infinite energy* to form a single one.

In the next chapter we will see, by reference to Dubuat's experiments, how far theory is confirmed by these experiments.

SUMMARY

Vortex rings are formed by what Reusch calls a "staccato movement" of the fluid past a sharp edge. The word "sharp" must be taken not in an absolute, but in a relative sense; for rings can be formed when the edge is *not very sharp*. The fluid is cracked and the cross pressure assists in generating a *cyclic* movement. The visible part of a smoke ring may be described as the "skeleton" of the vortex, the complete vortex being melon-shaped. Smoke rings cannot, ordinarily, be formed when the air is driven through a tube whose length exceeds five times its diameter. For every smoke ring formed by a smoke-box, there is a "clear-air" ring formed in the box, and which travels backwards through the smoky air.

All these vortices, formed in water or air, are *necessarily*

coreless; but the hollow core is not necessarily a vacuum; it may be filled with air, water vapour, smoke, etc.

These "rings" cannot be cut; they travel along at their own speed, and are eventually dissipated.

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CHAPTER XIII

MOTION OF WATER IN REAR OF A BODY EXPOSED TO A STREAM—RESISTANCE AT VERY HIGH VELOCITIES

IT was shown previously that if a plate was fixed and exposed to a moving liquid—or if the body was moving and the liquid was at rest—the pressure on the anterior face was less than the hydrostatic. As the plate undoubtedly meets with resistance, it is evident that the *posterior* pressure *must be less* than that on the anterior face. Since, further, the pressure on the anterior face *diminishes* as the velocity *increases*, it is also evident that the posterior pressure must *also diminish*, and *more rapidly*, than it does on the anterior face; for experiment shows that the *resistance* of the plate increases very largely as the velocity increases.

To submit the matter to experiment, Dubuat made measurements of the pressure on the posterior surface of a plate (fig. 44) by reversing his thin box in the stream, with the following results:—

No. 1. (Centre)	.	.	.	=	- 12·7 lignes.
No. 2. (Half-way)	.	.	.	=	- 12·0 „
No. 3. (Three-quarters)	.	.	.	=	- 14·9 „
No. 4. (Close to edge)	.	.	.	=	- 15·7 „
No. 5. (Bottom corner)	.	.	.	=	- 15·3 „

As was to be expected, the pressure is *very much less* than it was on the anterior side. Attention may be directed, in examination of these figures, to a curious thing: though the pressure at the centre is less (*measured negatively*) than it is at the edge—as one would suppose it should be—still, the

progression is *not regular*. This might be considered as due to unavoidable experimental errors; but, against this, the experiments appear to have been conducted with very great care, and, also—*this peculiarity recurs in other cases*. What appears evident is that the water at the rear of the plate was in *very rapid motion*, and that the velocity opposite hole No. 1 was greater than opposite hole No. 2.

Now what should we expect if the thin box were fixed behind a cube A B C D (fig. 45)? We know that the pressure

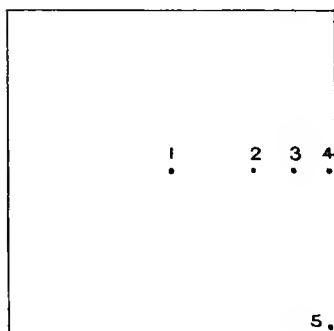


FIG. 44.

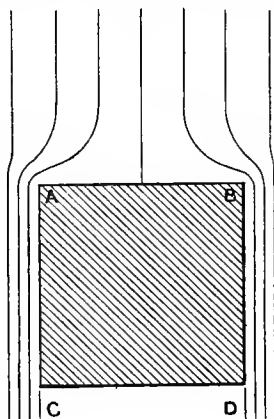


FIG. 45.

on the *face* of the cube is not, materially, different from that on the thin plate. There appears no reason to suppose that there should be any *essential* difference between the action at the rear of the plate and at the rear of the cube. Of course the water flowing along A C and B D must be somewhat retarded by the friction; so that the velocity of the stream-lines should be rather less at C and D than it is at A and B. The vortex formed at the back of C D ought, therefore, to be less strong than the one formed behind the plate.

On placing the thin box behind the cube and facing towards the rear, Dubuat found the following pressures:—

No. 1. = $-7\cdot2$ lignes.	No. 3. = $-8\cdot0$ lignes.
No. 2. = $-7\cdot1$ „	No. 4. = $-6\cdot0$ „ (P).

This confirms what we should have expected from theoretical reasoning. The progression is *again irregular*, that at the centre being in excess (*measured negatively*) of that opposite hole No. 2, thus confirming the results obtained behind the plate. The change of velocity (?) from 3 to 4 appears quite inexplicable. Dubuat notices this irregularity, which

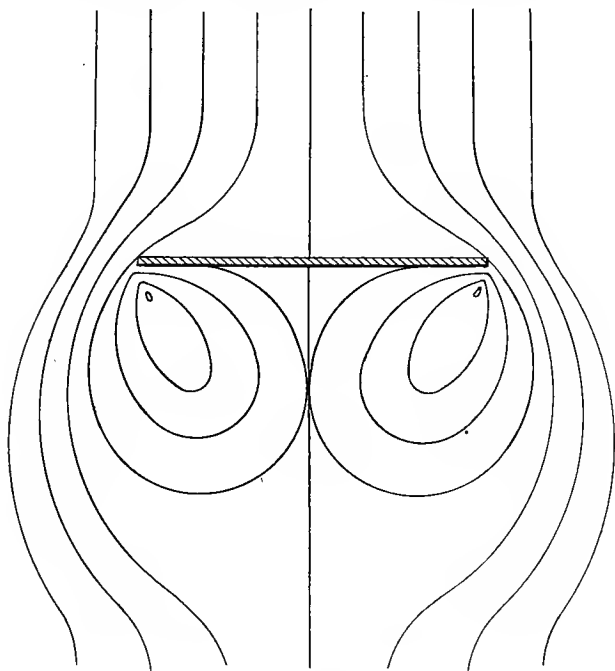


FIG. 46.

he calls “une marche très irrégulière, et même contraire. Mais cet effet peut avoir été produit par une faible déviation au trou du bord.” It seems a pity that he did not repeat the experiment more carefully.

Dubuat fixed the thin box behind a parallelepiped of proportions $3 \times 1 \times 1$, and found pressures at—

No. 1 (fig. 44). = -1.5 lignes. No. 2. = -3.2 lignes.

These figures tend to show that the strength of the vortex was considerably less than that behind the cube.

Judging from the pressures measured by Dubuat on the rear face of the plate, and remembering that any change in p involves an opposite change in V^2 , the vortex behind the plate must be *something like*, though possibly differing from, the sketch, fig. 46. It would be a form of what Professor Lamb would appear to call a "spherical vortex," with the core very close to the edge of the lamina. Since sketching out this vortex from theoretical reasoning, I have become aware that Avanzini in 1804 (*Istituto Nazionale Italiano*, Tomo i., Parte 1) found *experimentally* that the water actually moves in this manner. By an arrangement of silk threads attached to fine wires he found the direction of flow at very many points and then drew the sketch, in fig. 47 (traced from the original), of the vortical motion. The agreement between theory and experiment is most close.

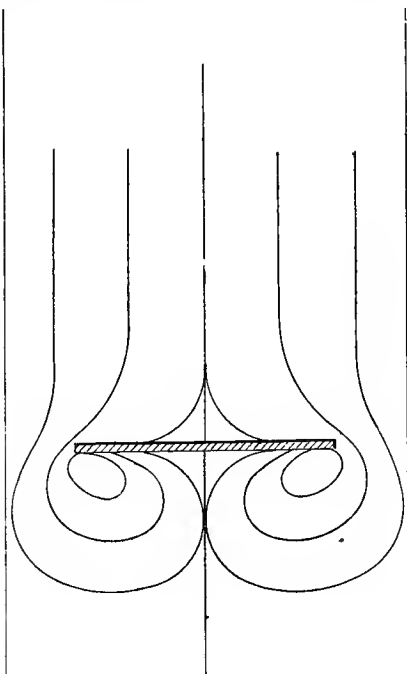


FIG. 47.

It was pointed out that in Dubuat's experiments the pressure at the back of the plate was "irregular"; that the pressure (*measured negatively*) increased from the centre to the edge, but that it commenced by *decreasing*, so that the negative pressure at the centre was greater than it was at hole No. 2 (fig. 44). This was very puzzling at first, as the only possible explanation was that the water was in more rapid motion in the centre than half-way towards the edge. If one considers, however, that the pressures recorded are those when the vortex *has just broken*—when the

vortex is *formed* it acts as an elastic solid—it will be evident that as the water flowing through the centre of the ring (fig. 48) has to spread out right and left (*viewed in section*, but in reality in all directions), it should be flowing faster in the centre than when it has “turned the corner,” so to speak; and this would account for the differences of pressure. This is not insisted on, but it appears a reasonable explanation; the records appear reliable, and this peculiarity recurs.¹

It is hardly necessary to point out that the pressure measured *behind the plate* is less (*measured negatively*) than it ought to be. Since vortices are being rapidly formed and broken, the pressure is not a constant one, but is varying rapidly. This variation is *damped*, and it is the “damped pressure” which is recorded and not the real negative pressure.

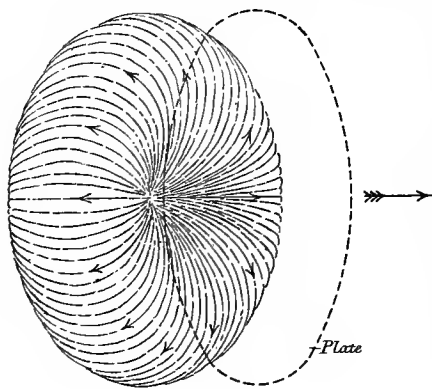


FIG. 48.

Since the strength of the vortex is dependent on the velocity of the stream-lines passing the edge of the plate, and this is dependent on the velocity of the stream, it will be apparent that the resistance caused by forming these vortices will vary *as the density* of the liquid and as the *square of the velocity* of the stream. The total resistance of the plate may, therefore, be expressed by the formula already discussed, *i.e.* $R = AV + BV^2$.

If we refer to Beaufoy's experiments on the resistance of a square flat plate: area = 2.9718 square feet; immersed 9 feet below a float. The figures given are the resistance of the combination *less* the resistance of the float and iron bar supporting the plate.

¹ Fig. 48 has been sketched from a model made by winding tarred string round a hollow curtain ring; the hollow in the ring representing the empty “core.”

RESISTANCE OF A SQUARE FLAT PLATE

Area = 2.9718 sq. ft. Immersion 9 ft.

R = Total resistance *less* that of float and bar.

V. Ft. per Second.	Resistance (observed).	$\frac{R}{V^2}$.	Resistance (calculated). A = .26, B = 3.515.
1	3.5925	3.5925	3.765
2	14.312	3.578	14.56
3	32.130	3.570	32.38
4	57.017	3.563	57.24
5	88.96	3.558	89.12
6	127.94	3.554	128.04
7	173.96	3.55	173.98
8	227.00	3.547	226.96
9	287.03	3.54	286.96
10	354.09	3.54	354.00
11	428.14	3.538	428.06
12	509.19	3.536	509.16
13.527	546.36	3.532	546.55

A = .26, B = 3.515.
(Beaufoy's experiments.)

An examination of the table, giving *observed* and *calculated* resistance, will show the correctness of the formula.

By a similar line of reasoning it can be shown that the resistance of *all* bodies moving in liquids may be expressed by the formula $R = AV + BV^2$; but, as we have seen, in the special cases where

1. The liquid (*supposed incompressible*) is of *infinite* extent, or
2. Completely fills a closed tank, the walls of which are inextensible;

(These two might be classed under one heading "When the liquid has no free surface," but are separated to make my meaning clearer.)

3. When the motion is excessively slow;
4. When the liquid is flowing through *very fine* tubes, or between plates *very close* to one another,

the inertia resistance does not exist. The term $BV^2 = 0$, and the resistance = AV only—the resistance *due to viscosity*;

which varies *as the shearing surface* (not, as usually stated, *as the wetted surface*).

A in the formula is not necessarily positive: there are cases where the resistance may be expressed as $R = BV^2 - AV$. For example, in Beaufoy's experiments (p. 474) we find the resistances of a globe of 13·5 inches diameter encountered at different velocities. If these be examined by the formula $R = AV + BV^2$ it will be found that $A = \cdot 245$ and $B = \cdot 328$ will satisfy the equation.

When the sphere was lengthened by the insertion of a middle piece 12 inches long the resistance was so much reduced that A became $= -\cdot 094$ and $B = \cdot 29$. But the discussion of this point lies outside the scope of this book.

As there is a "critical velocity" *below* which the resistance varies *as the velocity*, so also there is a *superior* "critical velocity" *above* which the resistance again increases in this ratio.¹ This, of course, follows logically from what has been said about the "law of flow." Referring to the formula $P_0 = p + \frac{1}{2}\rho v^2$, we know $p + \frac{1}{2}\rho v^2 = \text{constant}$, and that p cannot be *negative*. It follows that v has a maximum value, beyond which it cannot increase. The maximum value of v , in a gravitating fluid, varies as the depth of immersion; thus, if, at a given depth, the total pressure, hydrostatic and atmospheric, is P_0 , then the *maximum* value of v for this depth is found from the equation $\frac{1}{2}\rho v^2 = P_0$ (p having become *zero*) or $v = \sqrt{\frac{2P_0}{\rho}}$. When the velocity of the stream-lines, moving past the lamina, has reached this *maximum*, the strength of the vortex—and, *ergo*, the *minimum* pressure on the back of the lamina—*will remain constant* and the resistance will then be expressed as $= \text{const.} + AV$, when the increase of resistance is *as the velocity*.

This idea of the pressure remaining constant is not new, for Dubuat says:

"Quand l'effet de la non-pression est parvenu, par un certain degré de vitesse, à égaliser l'effort naturel de l'air, la poupe doit être entièrement vide, et le défaut de pression est

¹ C' in fig. 18, p. 66.

parvenu à son plus haut degré ; alors quelque augmentation que puisse recevoir la vitesse du mobile, la *non-pressure reste constante*" [*italics added*].

Experiment will be found to confirm this view. Some years ago, through the favour of Mr Yarrow, I was enabled to examine the H.P. speed curves of the torpedo boat destroyers *Havock* and *Sokol*. I found the "critical speed" was between twenty-four and twenty-five knots; beyond these speeds the resistance appeared to increase *as the velocity*, the increase in H.P. being very nearly the same *for each knot* above this speed. *Theoretically* the critical velocity should have been more than this speed; but then theory assumes the pressure in the vortices to be *zero*, which, practically, it never is. There is always a residual pressure.

This law holds also for air, where the "critical velocity" appears to be about 1100 feet per second—less than theory again. In the case of a bullet: "Beyond 1100 feet per second we may take P being in pounds, d the diameter of the shot in feet, v the velocity in feet per second, $F = fd^2(v - 800)$, where $f = 3$ for spherical and 2 for elongated shots with ogee-shaped heads" (Perry, *Applied Mechanics*), the resistance increasing *as the velocity*. In parentheses I may remark that *if Stokes' law holds for high velocities*—and there appears no reason why it should not—the resistance should vary as the *first power* of d and *not as the second*: certainly for the spherical shot, and I think equally for the elongated projectile.

Still more remarkable than this, I am inclined to believe (though I have much hesitation in making the statement) that at still higher velocities the resistance may even increase at a slower rate. There is indeed no *a priori* reason why the resistance due to viscosity should continue increasing indefinitely. Some of Professor Langley's experiments seem to support this view, for he says:

"Further than this [referring to the possibilities of flight], these experiments (and the theory also when reviewed in their light) show that if, in such aerial motion, there be given a plane of fixed size and weight, inclined at such

an angle, and moved forward at such speed, that it shall be sustained in horizontal flight, then *the more rapid the motion the less* will be the power required to support and advance it. This statement may, I am aware, present an appearance so paradoxical that the reader may ask himself if he has rightly understood it. To make the meaning quite indubitable, let me repeat it in another form, and say that the experiments show that a definite amount of power so expended at any constant rate will attain more economical results at high speeds than at low speeds, *e.g.* one horse-power thus employed will transport a larger weight at 20 miles an hour than at 10, a still larger at 40 than 20, and so on, with an increasing economy of power with each higher speed, up to some remote limit not yet attained in experiment, but probably represented by higher speeds than have as yet been reached in any other mode of transport" (Langley, *Aerodynamics*). It is not necessary, however, to press this point.

From the foregoing it will be evident that since inertia resistance is caused by the formation of vortices, and these vortices are produced at the edges or angles of a body moving in the liquid; then, the more sharp edges a body has the greater will be the resistance to its motion. In other words, a plate having more periphery—in proportion to its area—ought to experience greater resistance *per unit of area* than a plate of more compact form. It is well known that such is the case.

There are cases where resistance is an advantage. Mr John Bourne, C.E. (*Screw Propellers*), says: "In order that a vessel may be able to sail very close to the wind, the surface of the sail should be quite flat, *and the sails should have holes in them, or be made like a Venetian blind*, as the sails of vessels are made in China." I have been informed that in China some of the boats have rudders which are perforated. Mr Alexander has made a small model screw propeller with small holes in the blades. He says it gives a greater thrust than one without holes, though it obviously takes more power to drive it. Even 400 years

ago that great genius, Leonardo da Vinci, was convinced of the advantages of having small holes (*sportellini* is the word he used) in the wings of flying machines. No birds' wings are air-tight; bats, whose wings are, do not appear to be able to fly as steadily and gracefully as birds.

In conclusion, it will be seen that, *if examined under similar conditions*, the motion of an ordinary liquid does not differ very much from that of an inviscid liquid, if such a thing existed. The difficulties of the subject are chiefly caused by the assumptions made by the mathematicians, and the consequences leading from these assumptions.

1. The ideal liquid is supposed to be *continuous*. This leads to the maze of the *infinite negative pressure* at every sharp edge, which, of course, involves the assumption that the liquid is capable of sustaining *infinite tension*. This assumption appears unnecessary, and, I am afraid, against common sense. To assure that the flow *shall be continuous*, it is only necessary to have a sufficient *vis a tergo*—sufficient “push from behind”—to keep it so. If the “push” be not sufficient, the liquid (which I assume with a free surface), having inertia, will “tear” away and cause “discontinuity.”

2. The next assumption is “incompressibility.” This leads to even a worse tangle. In the first place, the deduction is that a stress would be transmitted *instantly* to an infinite distance; *per contra*, however, the stress *could not be transmitted at all*. How can one molecule transfer energy to another *incompressible molecule*? What, for example, would be the result if an *incompressible shot* were to strike an *incompressible target*?

3. *Inviscidity*. — It is difficult to find out what is generally meant by the term “viscosity.” If it means treaciness or stickiness, then it implies *attraction* between the molecules of the liquid. “Inviscidity” would then imply that there is *no attraction* between the molecules. Yet this same inviscid liquid is supposed to be capable of being under *infinite negative pressure*!

I am aware that some physicists define viscosity as “a kinetic effect, due to the interpenetration, in course of time,

of the various layers of molecules." This description, however, though very soothing from its profundity, leaves no very clear impression on my mind.

4. *Infinite size of the liquid.*—This has been dealt with at some length, for it appears to be the chief cause of most of the puzzles put before the student.

The subject of flat plates moving at an angle in liquids, the screw propeller (which is really only a modification of this), and some other peculiarities of liquid motion must be deferred to some future occasion.

SUMMARY

The resistance experienced by bodies moving in liquids is chiefly caused by the formation of vortices behind them; these vortices are being continuously formed and as continuously "peeled off" and destroyed by the new vortices formed in front of them. In the case of a circular lamina the vortices are of a spherical form, with the core very near the edge of the lamina, the particles of liquid, moving round the core, travelling in elliptical (?) orbits, like comets moving round the sun.

Since "discontinuity" is necessary for the formation of the vortex, it follows that if there is *sufficient vis a tergo*, or "push from behind," in the liquid to enable it to get round the corners of the body nicely, *there will be no inertia resistance.*

The laws that apply to inviscid liquids apply equally to viscid ones—special reservation made of the question of "rotation" *in the sense specially defined by me.*

There are no special laws which apply to "flat bodies" which do not equally apply to "round," or even "ichthyoid" bodies.

REFERENCE

J. BOURNE, *Screw Propellers.*

APPENDIX

It has been suggested to me that in these days, when everyone appears to be more interested in Aerodynamics than in Hydrodynamics, it might be useful to indicate the connection between the law of the resistance of liquids—incompressible fluids—and compressible fluids, like air.

It is commonly taught that (certainly up to speeds of 100 miles per hour) the laws of the resistance of liquids and gases is the same—subject, of course, to the densities being *roughly* as 800 : 1, and the coefficients of viscosity being different. Dr Stanton (*A.C.A.*, 1909-10) says: "In the case of models placed in a current of air, the resistance is found to be proportional to the square of the speed within ordinary limits, so that there is not the same necessity to make the tests on models at corresponding speeds." Mr F. W. Lanchester (*A.C.A.*, 1909-10) says: ". . . In connection with flight, or, more broadly speaking, with aerial navigation, *this effect of the compressibility of the fluid can be ignored.*"

It *is* true that the same laws apply when compression does not take place; but it is certainly *not* true that "the effect of compressibility can be ignored." In other words, under *similar conditions* it is true, but not otherwise.

To make my meaning perfectly clear, I will first refer to cases where the conditions *are* similar, and then to cases where they are not.

(1) "*The surface friction of thin plates.*—The most reliable work in this direction appears to have been done by Zahm. . . . The resistances bear a striking resemblance to those obtained by Mr W. Froude (Table II.) for plates in water, in that the resistance per square foot *diminishes with the length*, and for smooth surfaces varies as $V^{1.85}$, where V is the velocity" (Dr Stanton, *A.C.A.*, 1909-10).

In this case compression does not occur, or, to be accurate, only to an extent which is apparently negligible; the conditions are *exactly similar*. It is unnecessary to labour this case, for if the reader will refer to Chapter VIII., whatever was said for water will apply equally to air. What I have likened to the "bull-roarer" action is produced, and this accounts for the "resistance per square foot diminishing with the length." The resistance varies as $AV + BV^2$.

(2) When compression occurs this law no longer holds good; another term has to be added, when $R = AV + BV^2 + CV^3$.

In the bluebook on *Aeronautics for 1909-10* (which for brevity I refer to as *A.C.A.*), Lord Rayleigh has a note on "Dynamical Similarity," in which we find the formula

$$P = \rho v^2 \cdot f\left(\frac{v}{vl}\right) \dots A$$

for the pressure on a plate in normal presentation, which he gives as "the expression for the mean force per unit area normal to the plate."

ν = kinematic viscosity, l = length of side of plate, v = velocity, and "where $f\left(\frac{v}{vl}\right)$ is an arbitrary function of the *one* variable v/vl ."

It is for experiment to determine the form of this function, or in the alternative to show that the facts cannot be represented at all by an equation of form (A)" (*A.C.A.*, 1909-10).

In the report (*A.C.A.*, 1910-11) we read: "Messrs Bairstow and Booth have shown that a formula *can be found* [emphasis mine] falling under the general type indicated by Lord Rayleigh, which accurately represents the results, both of Eiffel and Stanton, over the whole range to which their experiments extended, when both the dimensions of the plate used and the air velocity at which the results were obtained are taken into account."

"The question is one which is at present mainly of theoretical interest, and the importance of which lies in the light it may throw on the comparison of water and air resistances."

In the appendices to the report (*A.C.A.*, 1910-11), when referring to resistance of square plates in normal presentation, Messrs Bairstow and Booth say: "After some trial and error, it appeared that a formula of the type $F = a(vl)^2 + b(vl)^3$ was a satisfactory empirical approximation." There is nothing specially

new about this type of formula, for it was employed by Colonel Duchemin, as far back as 1842, for the resistance of projectiles in the *Mémoire d'Artillerie*.¹ It was also used by Zahm (*Phil. Mag.*, 1901) in his paper on the resistance of bullets.

On page 29 (*A.C.A.*), Dr Stanton gives another formula,

$$F = \rho v^2 \left(A \frac{v}{vl} + k + B \frac{vl}{v} \right),$$

for the law of resistance in rough and smooth pipes. He adds: "It is interesting to notice that the dimensional relation for these artificially roughened pipes is precisely similar to that found by Mr Bairstow and Mr Booth, in regard to the experiments on the normal resistance of flat plates of different sizes, *i.e.* the relation *necessitates the introduction of the third term* in the above equation,"

$$\left[F = \rho v^2 \left(A \frac{v}{vl} + k \right) \right].$$

Since this equation of Messrs Bairstow and Booth refers to unit of area, we may put the whole force on the plate as

$$F = \rho l^2 v^2 \left(A \frac{v}{vl} + k + \frac{vl}{v} \right).$$

If I may then take the liberty of changing the v in the last term to ρ (for reasons which will be obvious—it will only necessitate change in the value of B , which is a constant), and substituting μ/ρ for v , we then get

$$F = \underbrace{\mu A(vl)}_{\text{(Stokes' law.)}} + \underbrace{\rho k(vl)^2}_{\text{(Newton's law.)}} + \underbrace{B(vl)^3}_{\text{(Term representing resistance due to compression.)}}$$

or, *very generally*, as previously stated,

$$R = AV + BV^2 + CV^3.$$

Since the first term depends on the "shearing area," it will be exceedingly small for a thin plate, and this formula will not differ very much (in this case) from

$$R = BV^2 + CV^3.$$

Why the resistance due to compression *should* vary as l^3 and as v^3 , I do not propose to enter into at present; it has, in reality,

¹ Not in the British Museum, and I know of only one copy in England.

nothing to do with Hydrodynamics, which treats of liquids which are *practically* incompressible.

Expressed in the same "dynamical similarity" notation, the resistance of liquids, and what is commonly called the "friction" of flat surfaces in gases,

$$R = \rho l^2 v^2 \left(A \frac{v}{\nu l} + k \right)$$

or

$$R = \underbrace{\mu A(lv)}_{\text{(Stokes' law.)}} + \underbrace{\rho k(lv)^2}_{\text{(Newton's law.)}}$$

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